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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

<div align="right">

J. B. Pinelli ad Mazonium.

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ALERE   FLAMMAM.
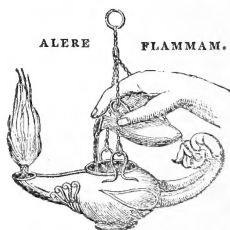
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NUMBER XXI.—SUPPLEMENT.

PLATES.

I. *Contributions to the Theory of Luminous Flames.* By Dr. KARL HEUMANN [*].

UNTIL lately Davy's theory of luminous flames sufficed to explain all observed phenomena ; but recently our knowledge has been enriched by a series of observations which cannot be well brought into accordance with generally accepted notions.

For example, the observation of Frankland that the flames of hydrogen and carbon monoxide become luminous when these gases are burned under pressure, is opposed to the former view that the luminosity of flame is caused by the presence of solid particles which become intensely heated. Knapp's experiment, showing that the luminosity of a flame may be diminished, not only by admitting air, but also by a due admixture of nitrogen or carbon dioxide, cannot be explained on the supposition of an oxidation of the carbon previously suspended in the burning gas. For these and other reasons, Davy's theory must either be altered or replaced by a new hypothesis. The latter course does not appear to me to be required. In the following contributions I shall endeavour rather to develop this theory than to overthrow it. The genius of Davy is made apparent when we find that the discovery of so many facts since his day has in no way overthrown his theory, but has only rendered a development of it necessary in order to bring it into keeping with an advancing science.

I have generally in my remarks adopted a chronological arrangement of the various researches, because it is only thus that the course of thought which I have followed can be logically represented. A systematic arrangement would, it is true, place the simpler before the more complex conditions; but in the present case the latter, which occur chiefly in the case of luminous carbon-containing flames, are the more important and have been more studied.

The experience gained from the study of flames burning under complex conditions is tested and applied in the case of simpler flames, and so becomes a guide to the explanation of the conditions affecting luminous flames in general.

Diminution and Restoration of Luminosity in Hydrocarbon-flames.

In the greater number of researches which have hitherto been conducted upon the luminosity of flames, those flames have been principally examined the luminosity of which is to be ascribed to the presence of carbon, and methods of research in which the luminosity has been either increased or diminished have, for the most part, been adopted.

Such methods are open to many objections, the principal of which (viz. the introduction of various agencies acting now in this way, now in the opposite, during the same experiment) has been too much overlooked. By reason of this oversight, researches, leading to diametrically opposed conclusions, have been published; and since Davy's theory is no longer of universal application, we have been left without any means of bringing the facts concerning luminous flames into harmony with one another.

Frankland has broached the hypothesis that the luminosity of flame is not due to particles of suspended carbon, but is caused by the vapours of heavy hydrocarbons which radiate white light. Strong positive evidence in support of a view so much at variance with the generally accepted theory, could hardly be expected; and Frankland has relied principally upon the fact that we are acquainted with many luminous flames in which we cannot suppose that solid matter is present.

To the instances already known Frankland has added the interesting observation that hydrogen and carbon monoxide when burned in oxygen under a pressure of 10 to 20 atmospheres, yield a luminous flame affording a continuous spectrum, and also that the faintly luminous flame of alcohol becomes as bright as that of a candle when the pressure is increased to 18 or 20 atmospheres [*]. These experiments are

* Compare L. Cailletet, *Compt. Rend.* clxxx. 487.

not so convincing as might at first sight appear, inasmuch as
we know that the temperature of the flame is increased at
high pressures *, and also that at the temperature of the electric
spark many gases yield a continuous in place of a line spectrum.
The power of gases as regards emission of light also varies
considerably under these circumstances; and it does not ap-
pear that we are absolutely necessitated, as Frankland has
supposed, to ascribe the increase in luminosity to the increased
density of the gas, although doubtless this circumstance is
not without considerable influence.

The inquiry as to the nature of hydrocarbon-flames is quite
independent of the meaning which we may attach to these
appearances ; and if Frankland puts forward the above-cited
phenomena of combustion as analogies to guide him in views
concerning carbon-flames, no very forcible argument can be
really deduced from the examples, because, as W. Stein † has
pointed out, it cannot be shown that the reaction in luminous
carbonaceous flames *must* be an analogous one to that described
above.

Frankland's declaration that the soot must be regarded as
an accumulation of heavy hydrocarbons whose vapours are
condensed on the cold body brought into the flame, may be
regarded as almost confuted by Stein's objection that in this
case the soot must become gaseous at higher temperatures
(which is not the case).

New doubts arise concerning the prevailing theory when
we consider that the admixture not alone of air, but also of
nitrogen, carbon dioxide, or other completely indifferent gases,
with coal-gas causes a great decrease in the luminosity of the
flame. Hence we cannot trace the decrease in
luminosity solely to the more energetic oxidation of carbon
contained in the flame.

The experiments of Stein ‡ and R. Blochmann § allow us
to suppose that, the particles of carbon being more widely
separated by the admixed gases, the oxygen of the air is able
to oxidize them to carbon monoxide more quickly than under
the ordinary circumstances of combustion.

To the theory of these authors, viz. that diminution of
luminosity is a consequence of dilution, Wibel ‖ opposes the
view that the absorption of heat brought about by the admix-
ture of an indifferent gas is the sole cause of decrease in

* Ste.-Claire Deville, *Compt. Rend.* lxvii. 1089.
† *J. pract. Chem.* [2] viii. 401.   ‡ Ibid.
§ Lieb. *Ann.* clxviii. 355.
‖ *Deut. chem. Ges. Ber.* viii. 226.

luminosity. Wibel was led to this view by considering the fact that a mixture of coal-gas and air, nitrogen, or carbon dioxide, which burned with a blue flame, became luminous and burned with a smoky flame when the tube from which the mixture issued was strongly heated. In this case the dilution must have been increased by the increase of temperature ; nevertheless the flame became luminous.

In order to convince one's self of the justness of the conclusions which are drawn from this observation, it is necessary to examine somewhat closely the method adopted by Wibel in his investigation. He says, " A tube of platinum, 8 to 10 centims. in length, is attached to an ordinary Bunsen's burner which is closed at the bottom; the gas to be burned is brought into the burner by means of a tube soldered to the lower part; when the flame of the burning gas has been adjusted to the proper point, the indifferent gas is admitted until the flame is rendered non-luminous ; the platinum tube is then heated by means of two non-luminous Bunsen flames held horizontally on either side of the tube, so as to ensure that it be equally heated . . . . The same appearance is noticed in the case of the ordinary Bunsen flame, rendered non-luminous by admixed air, when the platinum tube is placed in the opening of the lamp and is heated."

The last mentioned experiment, as described by Wibel, must be controlled before one can justly identify the diminution of luminosity in the Bunsen burner with Knapp's experiments upon diminution. Some time ago Barentin [*] showed that the amount of luminous gas which enters a given space is very different according as the gas is or is not ignited. Barentin believed that the explanation of the smaller amount of gas entering a burning lamp was to be found in the counter pressure exercised by the burning, and therefore expanding gas, upon the entering gas. Blochmann [†] showed that the diminished consumption of gas was to be traced solely to the increase of volume caused by the gas passing over the heated upper part of the burner.

The fact that a mixture of gases issuing from a Bunsen lamp through a strongly heated tube burns with a luminous flame, may therefore be due to causes other than that put forward by Wibel, viz. rise in temperature of the flame ; for it is evidently an improbable supposition that the consumption of gas, and therefore also the quantity of air (or other gas) drawn into the burner, will be unaffected by the passage of the gas over a glowing tube ; and so also it cannot, *à priori*, be expected that the proportion between air and gas will re-

* Pogg. *Ann.* cvii. 183.   † *J. für Gasbeleuchtung*, v. 355.

main the same when the mixture is passed over a hot as when it is passed over a cold tube.

I was therefore compelled to alter the conditions of experiment in order to render void that particular effect of the hot tube which has just been described.

If rise of temperature of the flame is the cause of increased luminosity, the effect must be the same if one heats, not the gaseous mixture, but only the indifferent gas. This experiment may be readily carried out with a Bunsen's burner, through the two air-tubes of which are passed small platinum tubes about 7 centims. in length ; the outer openings of these tubes are narrowed so that a quantity of air just sufficient to bring about complete non-luminosity is allowed to enter.

A thin-walled glass tube, the upper rim of which is covered with platinum, may, with advantage, take the place of the ordinary metal tube of the Bunsen's lamp ; the latter tends to cool the heated air to too great an extent. On lighting the gas issuing from the glass tube it burns with a non-luminous flame ; but on strongly heating the two platinum tubes by means of Bunsen's burners (care being taken that the products of combustion do not enter the platinum tubes) the flame becomes luminous.

It might be supposed that this fact is to be explained on the supposition that the volume of air passing over the platinum tubes is unaltered by heating these tubes, but that the true quantity of air calculated for equal temperatures is much smaller when the tubes are hot, and that there is therefore a deficiency in the amount of oxygen required to completely burn the carbon, and so to maintain the flame in a non-luminous state.

In order to show that this supposition is untenable, and that the increase in luminosity is to be ascribed solely to the rise in temperature of the flame, the experiment must be modified. Coal-gas and air, or carbon dioxide, are mixed in a gasometer in such proportions that, when conducted through a platinum tube about 10 centims. in length and 8 millims. in width, the mixture burns with a clear blue flame. If the platinum tube be heated to redness, the flame becomes nearly as luminous as that of ordinary coal-gas. On allowing the platinum tube to cool, the flame again becomes non-luminous.

From this experiment the conclusion is evidently to be drawn that it is *the added heat alone* which has caused the flame to become luminous, inasmuch as a diminished supply of air cannot in this case, as in the former, have influenced the result.

It remains, however, to be investigated whether the gaseous

mixture, burning with a luminous flame in consequence of the application of heat, has or has not been altered so that its luminosity shall continue when it has been allowed to cool to the ordinary temperature.   In other words, if the increase in luminosity is directly due to increase in temperature of the flame, and is not brought about by a chemical change in the gaseous mixture, then the flame which appeared luminous at the point of the strongly heated tube should again become non-luminous when the gaseous mixture is cooled, after having passed through the heated tube, and is then ignited.

This experiment may be carried out by connecting two glass tubes by means of gypsum to the platinum tube, the outer glass tube being V-shaped and being surrounded by cold water.   If the mixture of gases be passed through this arrangement, the platinum tube being strongly heated, and be ignited at the orifice of the glass V-tube, a non-luminous flame is noticed ; whereas if the V-tube be removed and the gases be ignited at the orifice of the platinum tube, the flame becomes luminous.   More simply, the experiment may be carried out by burning the gaseous mixture as it issues from a platinum tube about 12 or 15 centims. in length : in heating this tube near to its orifice the flame becomes luminous ; but on heating the tube at a point further back the luminosity of the original flame is not increased, because the heated gases are again cooled by passing over the outer part of the platinum tube.

In employing a mixture of air and coal-gas under certain conditions, it is found, as Wibel has noticed, that "the gas aspirated from the opening of the burner reveals—by the amount of water and carbon dioxide which it contains, as also by its burning with a luminous flame under the ordinary conditions"—that a partial decomposition has taken place.

While Wibel noticed a not inconsiderable deposition of carbon when air and coal-gas were passed through a red-hot platinum tube, in my experiments, in which the air only was passed through a heated platinum tube, no such deposition was noticed in the glass tube, at the orifice of which the gases burned with a luminous flame for a considerable length of time.   In Wibel's case the deposition of carbon was doubtless due to a too great local heating of the platinum tube through which the gases were passed.   Such an intense heating is not necessary in order to attain the aim of the experiment.   From that experiment in which the flame of a gas, previously rendered non-luminous, was restored to luminosity by means of heat, Wibel draws very far-reaching conclusions.   He rejects the deductions of previous experimenters ; but in doing so he rushes too far to the opposite extreme.   For example, he believes himself justified in concluding :—

" 1. Decrease in luminosity *cannot* be due to dilution of
the gases, whether understood in Frankland or Blochmann's
meaning of the term, inasmuch as in the above mentioned re-
searches such dilution was at any rate increased by heating,
yet the flame became luminous."

" 2. Decrease in luminosity, in Knapp's experiments, as
also in the case of the ordinary Bunsen's flame, is much more
to be traced to the *cooling effect*, on the interior of the flame, of
the entering gas. By heating the latter the flame becomes
luminous."

Wibel finds " a most noteworthy argument " in favour of
these two points in the peculiar behaviour of the flame of
coal-gas and oxygen.

On the one hand, this flame becomes non-luminous only
when the current of oxygen is rapid, and when the flame is
cooled by metallic gauze ; on the other hand, by proper treat-
ment the flame may be made a source of intense light.   These
circumstances show, according to Wibel, that neither dilution
nor oxidation is a cause of decrease of luminosity.   By similar
reasoning it might be shown that Wibel's theory is itself
erroneous.   Everyday experience tells us that the blue flame
of Bunsen's burner, as well as that of the blowpipe, possesses
a much higher temperature than the ordinary luminous flame ;
but if Wibel be correct in saying that decrease of luminosity
is a consequence of cooling only, then, logically, the tempe-
rature of the luminous flame ought to be higher than that of
the non-luminous flame.

Those flames whose luminosity is decreased by means of
air might perhaps not be classed with those in which a similar
result is brought about by means of indifferent gas ; but little
would thus be gained, for the Bunsen's flame behaves, so far
as its power of becoming luminous is concerned, similarly
to Knapp's flame, the only distinctive point (the higher
flame-temperature consequent upon the entrance of oxygen
in the admitted air) not being proportionately altered by
heating the tube of the burner, the flame nevertheless
becoming luminous.

But while, in the case of flames rendered non-luminous
by indifferent gases, it might be supposed that the heat gained
when the tube of the burner is warmed merely serves to re-
place that lost by absorption into the entering inert gas (which
heat had formerly caused luminosity), this supposition is con-
tradicted by the already cited analogous case of decrease of
luminosity by means of air, inasmuch as it cannot be supposed
that there is a withdrawal of heat from the luminous material
in the flame, the temperature of which is greatly increased.

In experiments upon decrease of luminosity caused by completely indifferent gases free from oxygen there will, of course, be a considerable decrease in temperature, because a fixed quantity of heat must be divided throughout a larger volume of gas.

W. Stein*, however, has pointed out that in these cases a cause other than lowering of temperature is at work. He shows that a flame rendered non-luminous by means of nitrogen yet possesses so high a temperature that it is able to decompose, with deposition of carbon, coal-gas conducted in a glass tube through it; he also observes that an inflammable gas, carbon monoxide, whose pyrometric effect is nearly as great as that of coal-gas, causes the flame of the latter gas to become non-luminous. In order to bring about the complete non-luminosity of 1 volume of coal-gas, there is required 1·6 volume in Bunsen's burner, and 0·9 volume in Brönner's burner, of carbon monoxide.

In this case decrease in luminosity is not accompanied by a real decrease in the temperature of the flame; and we are obliged to allow that dilution of the burning gas plays an important part, and may *of itself*, independently of any absorption of heat (which often takes place simultaneously) cause decrease in luminosity.

Wibel's experiment does not prove, as that author supposes it to do, that cooling of the interior of the flame is the sole cause of decreased luminosity, because the flame is simultaneously altered in its composition, *i. e.* it is largely diluted by the entering gas.

We find, then, some of those observers who have been already mentioned tracing decreased luminosity, brought about by admixed gases, *solely to the diluting action* of these gases; we find Wibel, on the other hand, tracing this decrease *solely to the cooling action* of these gases; but it appears to me that the truth lies between these two conflicting views.

It is difficult to devise experiments in which two or more causes tending to decrease luminosity are not simultaneously at work; and yet every thing depends upon our being able sharply to distinguish between these various causes. It will only be possible to gain a clear knowledge of the processes going on in flames when we are able to separate these processes and to study each alone.

It appeared to me necessary to devise an experiment in support of the well-known statement—the luminosity of a flame is diminished by cooling—which should admit of no other interpretation than this.

By the following method I have been able to show that

* *J. pract. Chem.* ix. 183.

cooling a flame is of itself capable of bringing about decrease of luminosity, and that luminosity may be then restored by simply applying heat; the result cannot be called in question by supposing dilution or oxidation to have taken place.

A luminous gas-flame, 3 to 4 centims. in length, proceeding from the point of a blowpipe or other narrow tube, is allowed to play horizontally upon a platinum basin suspended in a vertical position, so that the flame may broaden out and become blue. In this well-known experiment decrease of luminosity must not be traced solely to withdrawal of heat by means of the metal, inasmuch as the broadening out of the flame enables oxidation and dilution, as well as cooling, to influence the result.

If the platinum basin be now heated, on the side opposite to that on which the flame impinges, by means of a Bunsen's lamp held horizontally, the gas-flame becomes more and more luminous as the temperature of the basin increases, until it finally is restored to its original degree of luminosity. Of course the metal must be perfectly pure, and must not be touched with the fingers before the experiment; else the flame will be coloured yellow.

It is here shown that luminosity of the flame, which had been diminished by the use of the platinum basin, is restored solely by raising the temperature.

If the Bunsen lamp be removed, the flame quickly decreases in luminosity until it becomes blue.

In *this* experiment, in which decrease of luminosity is brought about by lowering the temperature, the objection formerly raised—viz. that the broadening out of the flame complicated the result—can no longer be maintained, inasmuch as the small decrease of volume consequent upon the cooling would tend to produce an opposite result. It is therefore experimentally proved *that cooling a flame is itself sufficient to cause a decrease in the luminosity of that flame.*

Reasons have been already given which oblige us to acknowledge that *dilution* of a flame by admixed gases is of itself sufficient to cause decreased luminosity * (Bunsen's flame, decreased luminosity by carbon dioxide); and inasmuch as the admission of a cold gas into a flame must withdraw heat from that flame, it is concluded *that the decrease in the luminosity of carbon-containing flames brought about by*

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* Frankland has observed that decrease of luminosity of carbon-containing flames is a consequence of dilution by lowering of atmospheric pressure; and he has concluded that the decrease of luminosity is connected with the decrease of pressure. I have not cited this experiment in proof of the effect of dilution in decreasing luminosity, because lowering of temperature is associated with lowering of pressure, and this must have an influence in decreasing the intensity of the light.

admitting indifferent gases is due to dilution, and also to lower-
ing of the temperature of the flame by these gases.

The fact that a flame which has been rendered non-luminous
by means of indifferent gases may be again rendered luminous
by heating the tube of the burner, I hope to explain by es-
tablishing the following points.

A flame formed of coal-gas and an indifferent gas or air,
and burning blue, requires, in order to cause it to become
luminous, a higher temperature than that which is possessed
by the luminous undiluted flame. The flame of a Bunsen's
burner in which non-luminosity has been brought about by
means of air is very hot, but becomes luminous when the
temperature is much increased by heating the tube.

These points in the behaviour of the flame of coal-gas and
oxygen, which Wibel adduced in support of his theory, are
explained by me as follows. Blochmann and Wibel both
noticed that the luminous flame of a Bunsen's lamp, fed with
oxygen by one opening while the other is closed, can be rendered
non-luminous only by employing a rapid current of oxygen
and a cooling surface of metallic gauze, simply because the
temperature of the flame, when pure oxygen is employed, is
very high. The absorption of heat caused by the entrance of
cold oxygen, as also the absolute rise in temperature required
by the *gaseous mixture* in order that it shall become luminous,
are entirely, or almost entirely, equalized by the intense heat
produced by the combustion in pure oxygen. Therefore the
production of non-luminosity is so difficult; that non-lumino-
sity should be brought about only by employing a rapid stream
of oxygen and a cooling metallic surface is self-evident.

It is known that a gas-flame may be caused to burn with
great luminosity by the admission in proper quantity, and by
a proper method, of pure oxygen. This fact certainly depends
upon the production of a very high flame-temperature unac-
companied by such dilution as is noticed in the Bunsen's or
blowpipe flame when air is employed, and when the diluting
gas is nitrogen. In this experiment it is found that the
greatest luminosity occurs when a rapid stream of oxygen is
introduced, but that too great a quantity of oxygen, as too
small a quantity, tends to decrease luminosity. Inasmuch as
a much higher temperature might be reached by increasing
the quantity of oxygen beyond that at which the maximum of
light is evolved, it seemed probable that the actual action of
this excess of oxygen in decreasing luminosity was not to be
traced solely to its cooling and diluting the burning gas, as is
the case with altogether indifferent gases, but that a third
cause, perhaps more energetic than either of those just men-
tioned, was at work.

This supposition led to a more exact examination of the changes brought about in the flame of coal-gas by an excess of oxygen. When a flame, burning at the orifice of a wide tube, is placed in an atmosphere of pure oxygen, a notable increase in luminosity takes place within the flame-mantle, which is itself, nevertheless, considerably decreased in size, while the outer non-luminous border of the flame is broadened out. In order to study this action more narrowly, I have found it advantageous to make the flame very small by allowing the gas to issue from a narrow tube. If, for instance, a flame of coal-gas 4 to 5 centims. in length, issuing from a blowpipe-nozzle, be plunged into a reversed jar of oxygen, the appearance of the flame is greatly altered. The outer, scarcely visible, part of the flame increases enormously in size at the expense of the inner and luminous part. A small luminous point alone represents what was formerly a broad luminous band ; at the same time, the whole flame decreases proportionately from what it had been in air. This is to be accounted for by the absence of diluting nitrogen, a circumstance which also causes the temperature of the flame to increase considerably. The decrease in luminosity can scarcely be traced to any other cause than the large quantity of pure oxygen, which, by diffusing inwards into the narrow flame, brings about an immediate oxidation of the contained carbon, which is, therefore, not necessitated to spread through the flame in a red-hot state in order to find oxygen sufficient for its combustion.

If this supposition be true, it follows that decrease of luminosity can only be brought about by combustion in oxygen in the case of those flames the light-giving constituent of which is capable of being converted by excess of oxygen into a feebly luminous gas, but that those flames the luminosity of which is due to some substance which cannot be transformed by oxygen into such a gas must continue to burn in oxygen, even when issuing from the *smallest* orifice, with brilliancy— that, indeed, an increase in luminosity must be brought about under such conditions, because of the increased temperature of the flame.

Direct experiment confirms these deductions, and therefore also the original supposition.

Hydrogen saturated with vapour of chromium oxychloride ($Cr_2O_2Cl_2$), and issuing from a blowpipe-nozzle, burns in oxygen with a dazzling white light : the luminosity is in this case due to the presence in the flame of chromium oxide. If the hydrogen be laden with the vapour of stannic chloride ($SnCl_4$), it burns, under the same conditions, with a blue flame of much

greater brilliancy than when the combustion proceeds in ordinary air. The product of combustion is in this case also a solid, viz. stannic oxide.

In order to prove that a similar appearance is noticeable in the case of luminous vapours, in so far as these are not oxidized to non-luminous gases by excess of oxygen, hydrogen was conducted through a vessel containing common salt and zinc filings moistened with dilute hydrochloric acid (as in Bunsen's well-known experiment). The gas issued from a blowpipe-nozzle and burned with an intensely yellow flame, the luminosity of which was not decreased, but rather the reverse, when the flame was plunged into a vessel containing oxygen.

Inasmuch, therefore, as the decrease in luminosity which a small coal-gas flame suffers when burned in oxygen is due to the presence of an excess of the latter gas, the fact that this decrease does not take place to so marked a degree when the flame is burned in ordinary air is to be traced to the presence of inert nitrogen, which, by diluting the oxygen, diminishes the energy of the oxidation.

In order to prove the justness of this conclusion, the nitrogen in a given volume of air was replaced by carbon dioxide ; *i. e.* a cylinder was filled over water with 1 volume of oxygen and 4 volumes of carbon dioxide ; and, after carefully mixing the gases, a coal-gas flame, burning at the orifice of a small brass tube, was brought into the mixture. The flame continued to burn with a degree of luminosity equal to that which it exhibited in ordinary air ; it follows, therefore, that the fact of dilution alone influences the result, the nature of the diluting gas being unimportant.

Every indifferent gas, including the products of combustion themselves, must exert a similar influence. When an ordinary flame, issuing from a fine orifice, is burned in oxygen, the luminosity decreases for the reason formerly assigned ; but as soon as the products of combustion (water and carbon dioxide) accumulate sufficiently to dilute the oxygen considerably, the luminosity begins to increase. The flame which had been reduced to a luminous point becomes enlarged until it presents an appearance similar to that exhibited by it when burning in ordinary air ; this happens at the moment when the oxygen in the vessel is diluted by the products of combustion to the same proportionate extent as it is diluted by nitrogen in the atmosphere.

If the combustion be continued beyond this point, the luminosity again decreases—not as was noticed in the former case, by a great decrease in the size of the flame-mantle, but by

general weakening of the light until complete non-luminosity is attained.   The flame then increases in size, and finally goes out.

This kind of non-luminosity exhibits a great resemblance to that noticed when the burning material is diluted by mixing with it indifferent gases, such as carbon dioxide and nitrogen; the causes of non-luminosity are indeed in both cases identical.   Inasmuch as every ordinary flame (with the exception of the flames of explosive substances) requires for its existence two combustibles, the chemical union of which brings about the glowing of the gases, it follows that it is a matter of indifference which of the combustible materials is diluted by indifferent gases—the coal-gas for example in Knapp's experiments, or the oxygen of the atmosphere.

And in fact it may be shown that a gas burning with luminosity in ordinary air, burns with a blue flame when plunged into a mixture of 5 volumes of air with 2 volumes of carbon dioxide. This experiment is the converse of Knapp's; and, as in that case, decrease of luminosity is due to dilution, and cooling of the flame.   Instead of diluting the air with carbon dioxide previously to the experiment, the products of combustion may be allowed themselves to bring about this dilution, the gas being burned in an inverted globe : it is then noticed that the flame quickly becomes less luminous and then burns blue, at the same time increasing in size.

The flame remains non-luminous and yet large, but again becomes luminous if transferred at the proper moment to the atmosphere ; otherwise it goes out.

This experiment on decreased luminosity is perfectly analogous to that described in the case of a flame burning in oxygen; only in this instance nitrogen was absent, and the products of combustion were the sole diluents of the oxygen.

When a small gas-flame is plunged into an inclosed volume of oxygen there is noticed, then,

1. Decrease in luminosity of the flame, accompanied by increase in the size of the flame, the light from which is very small ;

2. Increase in luminosity commences, and proceeds until the flame exhibits an appearance similar to that which it possesses in ordinary air, because the energetic oxidizing action of the pure oxygen upon the glowing matter in the flame is moderated by the diluting products of combustion ;

3. A general decrease in luminosity ensues, but now by a lowering of the intensity of light of the whole flame, brought about by the increasing dilution of the oxygen by the pro-

ducts of combustion, and also by lowering of the temperature of the flame.

These two causes gradually increase, until the flame, which continually increases in size, becomes blue, then invisible, and finally, being cooled below the point of ignition, goes out.

Besides cooling and dilution of the carbon-containing flame, a third cause has been shown to influence the decrease of luminosity—viz. *the energetic destruction of the luminous material, i. e.* the oxidation of carbon to feebly luminous gases (carbon monoxide and dioxide).

Generalizing the results of the experiments upon the means by which flames which have become non-luminous may be again restored to luminosity, we find:—

1. That hydrocarbon flames which have lost their luminosity by withdrawal of heat again become luminous by the *addition of heat.*

2. That flames rendered non-luminous by dilution with air or indifferent gases become luminous by *raising their temperatures.*

3. That flames rendered non-luminous by excess of oxygen, which brings about energetic oxidation of the carbon, are again rendered luminous by *diluting the oxygen with indifferent gases.*

It would be very interesting to observe whether flames rendered non-luminous by admixture of indifferent gases may be again rendered luminous by heating the tube of the burner, the combustion being carried out under such pressures as would cause the molecules of the burning gases to maintain their original proximity to one another, notwithstanding the admixture of nitrogen or carbon dioxide. I have not myself the necessary apparatus at hand ; but I would direct the attention of any chemist who is interested in these experiments upon luminous flames to the subject. It would also be well to note whether the decrease in luminosity suffered by a small gas-flame when burned in oxygen is maintained when the oxygen is diluted to one fifth or further.

If, in the case of previous observers, the point of dispute was whether cooling *or* dilution were the cause of decreased luminosity in carbon-containing flames when the combustible material was mixed with air or indifferent gas, to me it appears that there are at least *three* causes, each of which is capable of decreasing the luminosity of these flames, viz. *withdrawal of heat, dilution, and oxidation of the luminous material.*

In most cases two or all of these causes are at work :—in

non-luminosity brought about by nitrogen and carbon dioxide, especially dilution and heat-absorption; in the widening out of the flame caused by a cold surface, absorption of heat and more rapid oxidation of carbon; and in non-luminosity caused by air, each of the three causes is at work.

In the latter case the presence of the oxygen of the admitted air tends to cause a rise in the temperature and a diminution in the size of the flame, circumstances which are opposed to the absorption of heat and dilution of the flame.

The flame of the Bunsen's burner appears to be the final product of a whole series of causes acting some in one direction, some in another; and it is not to be wondered at that observers of luminous flames have arrived at such diverse and contradictory conclusions, inasmuch as they have made the study of this flame their principal object, overlooking the great complexity of the conditions affecting it, instead of preceding such a study by an investigation of more simple instances of combustion.

Effect of Withdrawal of Heat upon Flames.

On account of the simpler conditions affecting so-called non-luminous flames I have considered these first, omitting all mention of changes in the intensity of light, until a study of the effect of the withdrawal of heat shall have given us some exact knowledge concerning this cause of decreased luminosity.

Distance between Flame and Burner.

In a paper of Blochmann's* the fact is noticed that a gas-flame does not touch the rim of the burner, nor a candle-flame the wick. Blochmann says:—" If a gas-flame be closely examined it is seen not to rest immediately upon the opening of the burner. In the case of a highly luminous flame the luminous portion presents too great a contrast to enable one to notice this fact with certainty; but by decreasing the quantity of gas the space between burner and flame becomes more apparent in proportion as the intensity of the light diminishes. The small semicircular non-luminous flame issuing from a bat's-wing burner when the supply of gas is small, may be arranged so that the space between the burner and the flame shall appear as great as the height of the flame itself."

This small intermediate space may be proportionately increased by mixing an indifferent gas, such as nitrogen or carbon dioxide, with the coal-gas before the latter is ignited. Blochmann also noticed that the intermediate space was increased by burning the diluted coal-gas under diminished

* Liebig's *Annalen*, clxviii. 345.

pressure ; and he concluded that the cause of this increase was to be traced to the presence of the diluting gas. He supposed that there is a "momentary combustion taking place in the lowest part of every flame ;" this can only be when the issuing gas is mixed with a due proportion of air ; therefore Blochmann supposed that the explanation of the increased distance between flame and burner, which is observed to take place when coal-gas is diluted with an inert gas, was to be found in the following statement:—"The greatly diluted gas issuing from the burner at once becomes mixed with air. In order to maintain the constancy of the flame this mixture must contain a fixed quantity of combustible gas. But that this quantity may be maintained, in the case of a diluted gas, at the same point as if the diluting gas were absent, a much larger volume of the issuing gas must become mixed with the air; that is, the space between flame and burner must be increased."

The following facts are, I think, opposed to Blochmann's somewhat strained explanation. *Where a cold object touches the flame, a dividing space, similar to that noticed between flame and burner, is always observed.* The colder the object and the more diluted the burning gas, the greater is the observed space.

If a flame be diluted with a considerable excess of carbon dioxide, for example, a piece of thick metallic wire brought into this flame causes a clear space around itself, which increases in proportion to the amount of carbon dioxide present.

This experiment is best carried out in a darkened room : it is always difficult to distinguish the limits of the very slightly luminous flame, even if a dark background be employed.

These facts point to the conclusion that *withdrawal of heat* from the flame by means of the upper part of the burner is the cause of the observed vacant space, and that to the same cause (withdrawal of heat) is to be assigned the *extinction* of the flame in the neighbourhood of a cold object. The explanation of the increase in the distances between flame and burner, or cold object, brought about by the presence of diluting indifferent gases, is to be found in the fact that the presence of such gases lowers the flame-temperature, by causing a partition of the quantity of heat needed to maintain a given quantity of the coal-gas in a state of combustion throughout a greatly increased volume of gas. If the temperature of the flame be already low, the further decrease occasioned by the introduction of a cold body, although small in actual amount, is sufficient to cool a *considerable extent* of gas beneath the ignition-temperature: the flame is therefore extinguished in this cold space.

If this be the true explanation of the production of the

observed vacant space, it follows that heating the object placed in the flame should cause a decrease in the extent of this space. The following experiments prove that this actually takes place.

A cold iron wire held in a non-luminous flame which has been diluted with an excess of indifferent gas, causes extinction of the flame throughout a considerable space around itself; but as the wire becomes hotter, this space gradually decreases in extent, until when the wire is raised to a red heat (either by the heat of the flame or by an extraneous source of heat), the flame is observed to rest upon the wire without any intervening space. Again, a mixture of coal-gas and carbon dioxide may be burned at the orifice of a platinum tube, so that a non-luminous flame, separated from the upper rim of the tube by a vacant space, is produced. If the platinum tube be now heated by means of a Bunsen's lamp near its orifice, the non-luminous flame spreads down throughout the formerly apparently empty space until it touches the platinum tube.

These experiments not only confirm the explanation already given, but they also completely exclude the possibility of any such cause as that suggested by Blochmann taking part, even to a subordinate extent, in the production of the space observed between the flame and the burner. For the experiments prove that a flame, even when largely diluted with indifferent gases, burns in contact with a heated burner; whereas an effect such as Blochmann imagined, tending to produce separation between flame and burner, although it might possibly be decreased, yet certainly could not be removed by heating the burner.

I therefore look on the following conclusion as perfectly just:—The fact that a gas-flame does not touch the ring of the burner, nor a candle-flame the wick—further, that a flame does not actually impinge upon a cold body placed within it, is caused by the withdrawal of heat from the glowing gas. The flame is cooled below its ignition-temperature ; it ceases to glow and becomes invisible: the flame in the neighbourhood of a cold body is extinguished.

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The experiment just described, which proved that a greatly diluted gas may be caused to burn in contact with the metallic burner when the latter is heated, leads us to inquire whether the action of the upper part of the burner in causing a separation between itself and the burning gas is not aided by the cold gas issuing from the centre of the burner, or, indeed, whether this cold gas is not of itself sufficient, under certain conditions, to produce the observed effect.

The temperature of the lower part of the flame is certainly not so high as that of the middle portions; and the cause of

this fact might be sought for in the presence of unburned and comparatively cold gas, which afterwards becomes heated at the expense of the lowest flame-mantle. It has been already shown that the distance between the flame and any object in contact with it is increased so soon as the temperature of the flame is decreased by the admission of indifferent gas. The cold unburned gas in the ordinary flame plays the same part, in reference to the lowest part of the flame, as the indifferent gas in the above-cited example did towards the burning gas in general. That the action of this cold gas in increasing the space between flame and burner is not, however, very great, is evident from the fact that, in an ordinary burner the vacant space alluded to is no greater, or not much greater, than that noticed between the flame and a metallic rod held in the upper part of the burning gas.

The foregoing observations are only applicable in the case of flames which burn under moderate pressures, as the flames of our ordinary lighting apparatuses—gas-burners, oil and petroleum lamps, candles, &c. If abnormal pressures are employed, the phenomena presented by the flames are greatly altered: in place of a space measuring scarcely 2 millims. from burner to flame, there is noticed a distance of very varying magnitude, generally to be measured in decimetres, the production of which is to be ascribed to quite other causes than those operative in ordinary flames.

An experiment has long been known in which spirit of wine is confined in a strong brass vessel furnished with an exit-tube and stopcock, and is then boiled until, when the stopcock is opened, the spirit rises towards the ceiling of the room : on bringing a flame near the exit-tube, the spirit burns with a luminous flame only near the ceiling, the stream of issuing liquid appearing non-luminous.

By boiling spirit of wine in a copper vessel, and causing the vapour to issue through a glass tube drawn to a fine opening about 3 millims. in width, a long flame is obtained the base of which is separated by a distance of 10 or 12 centims. from the orifice of the glass tube. This distance is diminished by warming the exit-tube, or by holding a small rod in the issuing vapour and thereby decreasing its velocity. A small drop of alcohol soon gathers at the opening of the glass tube ; if this be ignited by bringing a source of heat near it, or by causing the flame of the burning vapour to rush back by means of a rod held in the vapour, a small flame is produced which momentarily diminishes the distance between flame and burner ; but so soon as the little drop of alcohol is burned, the original distance is again assumed. According to a recent investiga-

tion of F. Benevides [*], the flame of strongly compressed coal-gas allowed to issue into the air, is separated by a space of several centimetres from the orifice of the tube whence it issues.   If the pressure amount to two atmospheres, and the tube be 45 centims. in length and 4 to 9 centims. in width, the distance between the orifice of the tube and the flame amounts to about 4 centims.   Benevides found the temperature of the dark space to be very low, which is only what one would expect.

The same author noticed that a flame brought near to the dark space was carried along by the stream of gas. This he regarded as proof of the dilution of the gas with air, caused by the surrounding atmosphere being carried along with the gas-stream which issued from the exit-tube with considerable velocity.   If a wire be placed in the flame and be moved backwards through the dark space, the flame also moves backwards towards the burner, but returns to its original position immediately the wire is removed.

Benevides looks on these facts as justifying the conclusion that the formation of the dark space is due to the mechanical action of the issuing gas, whereby the air is driven aside for a certain distance from the orifice of the exit-tube ; in this space the requisite amount of oxygen is therefore not obtainable by the gas, which consequently remains unburned.   If the exit-tube be very narrow and the velocity of the issuing gas be great, the pushing back of the air may become so intense as to render combustion impossible ; the flame is therefore extinguished.

I cannot profess to be satisfied with these explanations.   I cannot yet understand how the existence of the flame becomes impossible on the ground that the oxygen is driven back by the gas, and at the same time that the flame is extinguished through want of oxygen.   Such a condition is found *in the interior of every ordinary flame*, not in the flame of compressed coal-gas only, and is recognized as the cause of the low temperature of the interior of a flame, and of the fact that the flame forms a hollow cone of glowing gas.   This driving away of air occurs throughout a proportionately small space only, and on the *outer margin* of this space the chemical combinations constituting combustion take place.   These facts are so elementary that it would have been superfluous to mention them, were it not that Benevides has constructed a theory without taking them into consideration.   From the following passage one would derive a singular idea of the nature of flame; for if the phrase "l'action mécanique du gaz

* *Ann. de Chim. et de Phys.* [4] xxviii. 358.

sur la flamme" &c. be not taken in a figurative sense, Bene-
vides appears to regard the flame as a separate substance which
is carried along by the stream of gas:—" Lorsque l'on intro-
duit un solide, par exemple un fil métallique, on oppose une
résistance au mouvement du gaz, dont la vitesse diminue et
par conséquent, l'action mécanique du gaz sur la flamme qui
tend à la projeter à distance diminue aussi, d'où il résulte que
l'espace obscur diminue, et le jet lumineux se rapproche du
chalumeau."

In opposition to this theory it must also be remembered
that extinction of the flame could not be caused by the gas-
stream driving back the air, because combustion would always
be possible at the line of contact between gas and air. Out-
ward and inward diffusion would continuously tend to increase
the magnitude of the space where combustion was possible.
It is therefore quite impossible that the space noticed by Bene-
vides between the burner and the flame of compressed coal-
gas could be caused by the absence of oxygen, the oxygen
having been driven away by the stream of issuing gas. By
this removal of oxygen the inner cold portion of the flame
would be increased in size; and the flame itself would be
lengthened by the increased velocity of the gas-stream ; but
extinction could not be brought about at the outer limits of the
flame-mantle, as was noticed by Benevides.

The mechanical action of a rapid stream of gas upon the air
would also only cause an increase in the size of the flame, but
no removal of that flame from the burner.   I believe that one
cause of this removal is to be found in the *absorption of heat*
occasioned by the gas issuing with so considerable a velocity,
but that a second cause is also at work, viz. the relation be-
tween velocity of the gas-stream and velocity of propagation
of combustion—a circumstance which Benevides overlooked in
his theory, although he had apparently noticed it in his expe-
riments.

The cooling action exercised upon the lowest portion of the
flame by the quick inrush of gas may be divided into two
parts.   The temperature of the cylindrical flame-layer formed
nearest to the burner is lowered by the coal-gas in the same
manner, although to a smaller degree (on account of the low
conductivity of gas for heat), as when a metallic rod is held in
the flame.

The innermost portion of the burning layer, consisting of
coal-gas and air which has diffused inwards, may by this means
be cooled to such an extent as to be extinguished; in other
words, the ignited layer may be carried further from the point
where the gas issues, and an unburned mixture of gas and air
may take its place.

But besides this cooling action exercised by the gas itself, the temperature of the flame suffers diminution by means of the action of the cold air surrounding the stream of gas. The air which the issuing gas carries along with it not only tends to withdraw heat from the outer portions of the flame, but penetrates also into the flame-mantle, the temperature of which it therefore diminishes.

Such withdrawal of heat by means of the cold gas, and by means of admixed air, takes place in every flame, even when burned under small pressure; but the action of these two causes, especially of the latter, increases as the velocity of the gas-stream increases; and if this be great and the gas be also under high pressure, the flame may be so cooled in the neighbourhood of the burner as to be extinguished, and a mixture of air and unburned gas may be formed and carried forward on the surface of the issuing gas-stream. In this case the existence of a flame will become possible only at a considerable distance from the burner, where the velocity of the gas has diminished, and where therefore the ignited gas is not so greatly cooled.

By increasing greatly the velocity of the gas and by diminishing the orifice through which the gas issues, it may be possible to prevent the stream of gas from becoming ignited at all—as, for instance, it is possible to extinguish the flame of a gas issuing from a burner with a small velocity, the stopcock being partially closed, by fully opening the stopcock and so increasing the rush of gas.

The explanation of this extinction of the flame is to be found in the fact that the space between burner and flame is increased by cooling the gas, and that in this space an excess of air finds its way into the gas-stream, which, as it increases its distance from the burner, becomes more and more diluted with air, until at last the mixture cannot be caused to ignite. If the orifice be small this state of affairs is attained the sooner, because under such conditions the diameter of the stream of gas is small, and the gas therefore quickly becomes diluted with air. If the explanation which has been given of the fact that a rapid stream of gas burns only at some distance from the orifice whence it issues be true, it follows that the distance between burner and flame must be decreased by raising the temperature of the gas previously to its leaving the burner. I have been able to prove that this is the case by making use of the flame of alcohol-vapour already described.

A thin platinum tube, the length of one's finger, was attached to the glass exit-tube at which the alcohol vapour was burned. The alcohol was boiled, so that a space of 2 or 3 centims. intervened between the flame and the orifice of the tube. The

platinum tube was then heated by means of a Bunsen's burner held not too near to the issuing vapour, whereupon the distance between flame and burner gradually diminished until the two were in contact. On removing the Bunsen's lamp the original distance was quickly regained.

If the stream of gas be very rapid, the experiment carried out as just described does not succeed, because the temperature of the vapour in the tube is not sufficiently raised. I do not doubt that, in the experiment described by Benevides, the distance between flame and burner would be greatly diminished, if not actually removed, by passing the compressed gas through a long tube maintained at a full red heat before igniting it.

Although the explanation which I have given of the fact that a space is noticed between flame and burner in the case of quickly moving gases has taken into account all the points which have been observed, and although I have not found any facts opposed to this explanation, yet I must confess that I am scarcely altogether satisfied with it. Thus the fact that the approach of a small flame to the orifice whence the burning gas issues causes a diminution in the size of the observed space, is not to be set down so much to the decreased withdrawal of heat by the issuing cold gas (as was the case in the experiment with the heated platinum tube*), but much more to the carrying over of the combustion to the heated part. I cannot look upon the cooling actions described above as alone sufficient to cause all the observed circumstances.

The *second* explanation already given of the cause of the observed space in the case of compressed gases takes into account the relation existing between the velocity of the gaseous stream and the velocity of propagation of combustion.

In order to gain a clear idea of the action of this factor, let us suppose that the flame of a compressed gas issuing from a tube is separated by a distance of several centimetres from the orifice of the tube. The question suggests itself, Why does not the flame make its way backwards towards the burner? or, in other words, Why is not the combustion propagated backwards throughout the line of contact of gas and air towards the burner?

The gaseous stream is evidently surrounded by a zone consisting of a combustible mixture of air and gas molecules (I use this expression on account of its shortness). As soon as the temperature of a pair of molecules in one part of the zone is raised to the ignition-point by means of a flame brought near, chemical action occurs (combustion), and so much heat is thereby evolved as suffices to raise the temperature of the

* *Suprà*, p. 17.

neighbouring pair of molecules likewise to the ignition-point. This action is propagated throughout the mass, and continues so far and so long as the combustible mixture extends.

Such a process takes place in the combustion of all substances, whether solids, liquids, or gases ; and to this propagation of combustion is due the continuity of all flames.

Now, inasmuch as the gas existing between flame and burner in the cases noticed clearly consists of such a combustible mixture (which may be proved in the case of alcohol vapour by bringing a small flame to the orifice of the tube), it follows that the heat given out by the last pair of molecules actually undergoing combustion must act, in the manner described, upon the pair next them, and so on throughout the gaseous mixture; yet this does not appear to be the case. I say does not *appear* to be the case, because we are too liable to look on the flame as something having an existence of its own (see Benevides), and not to regard it as *a part of the gaseous stream, which is visible to us for a short distance.*

If we may forget for a moment the true nature of the gas, we might compare the burning stream to a rod placed in a fire, which glows in the central parts, the ends emitting no rays of light. In a magnesium lamp the metallic wire is kept in motion by means of clockwork : the position of the flame is thus maintained constant. If the wire is pushed out too quickly or too slowly, the flame is advanced or withdrawn; and a constant position is only maintained by moving out the wire at that rate at which the flame would recede were the wire immovable.

This recession of the flame is conditioned by the propagation of the ignition ; it becomes more rapid the higher the temperature of combustion and the lower the temperature of ignition of the combustible body. Thus a stick of phosphorus ignited at one end, and placed in a horizontal position, burns almost at once throughout its entire surface ; a longer period elapses before the ignition of a wick impregnated with petroleum is propagated throughout the length of that wick ; and if rape-oil be used instead of petroleum, the rate of propagation of ignition is yet slower.

Besides the difference between ignition- and combustion-temperature, two other points must be noted as conditioning the velocity of propagation of ignition : these are, the specific heat and the conductivity for heat of the burning body *.

So far as these are concerned, the withdrawal of heat from the issuing gas and admixed air is a circumstance which may

* The magnitude of the surface and the diameter are of consequence; but these may be eliminated by parallel trials.

be eliminated and which may be looked on as immaterial. But even without this, parallel experiments might lead to the discovery of interesting relations existing between the velocities of ignition and the combustion-temperatures of different combustible bodies.

For solid bodies (magnesium for instance) the velocity of propagation of ignition is equal to the velocity with which a wire of the substance must be moved forwards in order that the position of the flame may remain constant. The time required for the flame to travel to the end of a wire of known length might also be determined.

Easily combustible liquids might be placed in a hollow, and the time which expired between the ignition of one end of the liquid and the arrival of the flame at the other end noted. Liquids which burn only when absorbed by wick, might be so absorbed by wicks of known length, and the time required for the flame to travel throughout the length of the wick placed horizontally might be determined.

By the aid of such experiments a comparative quantitative expression for the liability to ignition of various combustibles might be gained.

For gases, the velocity with which the gas must issue in order to maintain a constant distance between burner and flame might be determined; or the distance might be measured, the velocity of issue being maintained constant. In order to do away with the changing velocity of different gases for the same distance from the burner (depending on the nature of the gas itself), it would be better to measure that velocity which is just sufficient to remove the flame from the burner.

I am here reminded of Bunsen's method for determining the velocity of ignition in the mixed gases evolved in the electrolysis of water[*]. The explosive mixture was burned at a small orifice of known area, the velocity with which the gas issued being gradually diminished by reducing the pressure until the flame passed backwards through the opening and ignited the mass of the gaseous mixture.

This point must be reached when the velocity of the issuing gas is an infinitely little less than that with which the ignition is propagated forwards.

Bunsen calculated the velocity of propagation of ignition, C, from the formula $C = \dfrac{4V}{\pi d^2 t}$ where V denotes the volume of gas issuing in t seconds, and d the diameter of the opening. In the case of the mixed gases from the electrolysis of water C was found to be equal to 34 millims. per second, while for

[*] Pogg. *Ann.* cxxxi. 165.

an explosive mixture of carbonic oxide and oxygen C was equal to less than 1 millim. per second.

These numbers cannot be made use of in the determination of the rate of propagation of ignition of a gas burning in air, inasmuch as in this case the admixed nitrogen and the abnormal conditions under which the combustible gas is mixed with air greatly diminish the rate of propagation.

In order to render clear the relations existing between the rates of issue and of propagation of ignition in the case of rapid gas-streams, the following considerations will be serviceable.

A burning gas obeys the law that the position of the base of the flame remains fixed when the rate of propagation of ignition is equal and opposite to the rate of issue of the gaseous stream. The latter is greatest close to the orifice of the burner, and decreases as this point is receded from, because of the opposition offered by the surrounding air. At all points where the velocity of the gas is greater than the velocity of propagation of ignition, the flame cannot exist of itself, because each gas-molecule will be carried to a point further than that to which the ignition is transmitted in the same time.

If, on the other hand, the rate of propagation of ignition is greater than the rate at which the gas-stream moves, the base of the flame will be driven back against the burner and will remain stationary at that point where the two velocities are exactly equal.

If a burning body be brought into the stream of gas, issuing under high pressure, at a *considerable distance* from the burner, the flame which is produced moves back against the stream of gas until it reaches the point defined above, where it remains stationary; if, however, the gas be ignited at the opening of the burner, the flame is carried along with the stream until the same point is reached. If the velocity of the gas-stream be increased, the flame moves further from the burner; if the velocity be diminished, the flame approaches the burner; and the flame rests quietly upon the burner only when the two velocities are equal, or when the velocity of propagation of ignition is greater than that of the issuing gas. The last-named condition holds in our ordinary luminous flames, the small distance generally noticed between flame and burner, or wick, resulting from the cooling action of the surroundings of the flame. The phenomena just described may be noticed in the flame of alcohol vapour issuing from an orifice with considerable velocity, as already described. If air be blown through benzol and a light be then brought to the mixture, a flame is produced which moves backwards or forwards as

the velocity of the gaseous mixture is increased or diminished. The same phenomenon may be well shown by passing carbon dioxide through ether contained in a vessel surrounded with warm water, and igniting the issuing mixture. The distance between flame and burner may, in this experiment, be altered either by altering the velocity of the stream of carbon dioxide, or by warming or cooling the vessel containing the ether.

Or the mixed gases may be caused to issue from a small balloon furnished with an exit-tube and stopcock: by slightly altering the pressure by means of the hand, the flame may be caused to move backwards or forwards; or it may be maintained in a constant position. If the exit-tube be of platinum, the flame may be caused to rest upon the orifice of this tube by heating the tube with a Bunsen's burner. Such flames then behave in a manner exactly analogous with that observed in the case of rapid streams of gas; and the explanations already given of the observed distance between flame and burner can be predicated of these flames, although diluted with carbon dioxide &c.; for the decrease in velocity of the gas is compensated for by the increase in the proportion of indifferent gases. The temperature of the flame is therefore low, and the withdrawal of heat by the indifferent gases considerable. The second explanation given of the distance between flame and burner, depending upon the different velocities of the gaseous stream and of the propagation of ignition, holds good in the case of these flames.

One might be disposed to raise the objection that in these experiments the gaseous mixture was not strongly compressed, and therefore did not issue with any great velocity. But it has been shown that the greater distance between flame and burner is a function of the difference of velocities of the gaseous stream and the propagation of ignition; and in the foregoing cases the latter must be very small, because the temperature of the flame is very low, and the molecules of carbon dioxide interspersed between the molecules of the combustible gas must carry away heat from the latter. In these flames, for the reasons just stated, the rate of propagation of ignition is small and is easily exceeded by the velocity of a comparatively slowly moving gas-stream, whence results the great distance between flame and burner. This explanation is rendered more probable by considering that experiment in which the distance spoken of was diminished by warming the ether through which carbon dioxide was passed.

Inasmuch as the volume of diluting gas was here proportionally diminished, the temperature of the flame was increased; the rate of propagation of ignition was also increased,

and therefore became equal to the velocity of the issuing gas at a point nearer to the burner than that at which these two velocities were previously equalized.

The diminution in the distance between flame and burner which was observed to take place in every case when the burner was heated, or when a wire was introduced between the flame and burner, must now be commented upon in the light of the second explanation already detailed. It is easy to understand why the distance in question should be diminished by heating the burner.

This distance depends upon the difference between the velocities of the gas and of the propagation of ignition ; and the latter is itself a function of the difference between the ignition and combustion temperatures.

The combustion-temperature is high because of the gas being heated previously to ignition ; the gas has been already heated *nearly* to its ignition-temperature. These two circumstances necessarily cause a considerable increase in the rate of propagation of ignition ; the velocity of ignition becomes greater than the velocity of the issuing gas ; and the distance between flame and burner is therefore diminished or entirely removed. The diminution in this distance which is brought about by holding a metallic wire between the flame and the burner, and moving the wire towards the latter, may be thus explained:—The flame-mantle is produced immediately behind the wire because the latter serves to shelter the flame from the cooling influence of the quickly rushing stream of gas. The heat so produced is communicated to the nearest portion of non-ignited gas, and the flame is thus caused to travel backwards towards the burner.

The familiar phenomenon of the flame of a petroleum-lamp burning above the slit in the piece of thin metal which surrounds the wick, is explicable on similar grounds. The flame is so cooled by the metal, at a small distance from the wick, as to be extinguished ; but the lower part of the petroleum-gas still continues to burn. A mixture of unburned petroleum-vapour and products of combustion of this vapour, therefore passes upwards through the slit. This mixture may be ignited by properly regulating the screw which raises the wick ; but the flame only appears at the distance of a few centimetres above the metallic cap. The velocity of ignition is very small, inasmuch as the combustible matter consists of heavy, easily condensable vapours, which are moreover greatly diluted by the products of combustion of the lower part of the gas, viz. by carbon dioxide and water, substances having high specific heats. The distance between the metallic cap

and the upper flame may be still further increased by cooling the combustible vapours. The following experiment is instructive :—

A glass tube, 8 to 10 millims. wide and about 10 centims. long, is fastened vertically in the middle of the slit in the metallic cap surrounding the wick of a lighted petroleum-lamp. By raising the wick a thick white vapour may be made to issue from the upper orifice of the glass tube. If this vapour be ignited, a small flame is produced, which plays above the smoke at a distance of perhaps 10 centims. from the tube. The products of combustion present above this flame are invisible, because the combustion is complete and the water which is produced is dissipated by the heat evolved.

If the column of visible vapour between the tube and the small flame be carefully observed, it is seen to be rendered transparent by the action of the heat radiated from the lower flame, and finally to become ignited. In this way the fact may be explained that the small flame does not rest directly upon the visible column of vapour, but is separated from it by a transparent space 1 or 2 millims. in extent. If the glass tube in this experiment be replaced by one made of platinum, and if this be heated, the small flame may be caused to approach and finally to rest upon the orifice of the platinum tube.

The column of smoke which is seen to issue from a petroleum-lamp burning without the glass cylinder, is caused by the cooling action of the metallic cap which surrounds the wick. The flame-mantle impinges upon this metallic cap, is thereby held back, and so is rendered unfit for propagating the ignition upwards. The lower flame, being fed by air entering from below, continues to burn, and produces new gases and vapours from the oil-saturated wick, performing, therefore, a part similar to that of the retort-fires in the manufacture of coal-gas.

As the metallic cap gets heated, the cooling action which it exercises upon the stream of ascending vapours diminishes, and the distance separating the upper flame from the lower is decreased. If the metallic cap be heated by a Bunsen's lamp, this distance becomes very small, and entirely disappears when the cap begins to glow. If a cap already heated to redness be placed upon a lighted and properly adjusted lamp, the flame does not become separated at all.

Everyday experience tells us that placing a glass cylinder upon the lamp causes the two flames to unite. The diminished supply of air brings about an elongation and curtailment in the dimensions of the flame, whereby it no longer touches the

sides of the metallic cap; at the same time the flame-temperature is increased, and the motion of the heated particles of gas is accelerated. These circumstances act in opposition to the cooling effect of the metallic cap.

These experiments may be interpreted as pointing to the withdrawal of heat from the sides of the stream of gas and air as the cause of the space noticed between flame and burner; but it has been shown that this action is but small, and that the superior velocity of the stream of gas over that of the propagation of ignition is the principal cause of the observed effect. Whether this be the sole cause cannot be determined until further experiments have been carried out.

The most important points established in the foregoing part of this paper may be summarized thus :—

1. The fact that a gas-flame does not rest upon the burner nor a candle-flame upon the wick, as also the fact that a flame never directly touches a cold body held within it, is to be explained by the cooling action exercised upon the gas by its surroundings.

The combustible gases are cooled throughout a definite space below their ignition-temperature; the flame is therefore extinguished. This conclusion is opposed to that of Blochmann.

2. The very considerable distance noticed between the burner and the flame of a gas issuing under high pressure, or mixed with a large volume of an indifferent gas, cannot be accounted for on the grounds put forward by Benevides. The production of such a distance is much rather to be traced to the cooling action of the stream of gas and of the outer air, and perhaps more especially to the fact that the velocity of the stream of gas in the neighbourhood of the burner is greater than the velocity of propagation of ignition within the gas.

3. In order that other circumstances conditioning the effect may be removed, the velocity of propagation of ignition must be *equal* to that of the gas-stream at the point, situated some distance from the burner, where the flame begins.

Determinations of the velocity of ignition should be made under these conditions for different gases; and since this magnitude is a function of the difference between ignition and combustion temperatures, conclusions may be drawn from such experiments regarding the relations existing between these points *.

* Since going to press, I have noticed an interesting paper by E. Mallard [*Annales des Mines,* 1875, iii. 355], in which the velocity of

4. The velocity of propagation of ignition may be easily determined for solid and liquid combustible bodies; and the numbers so obtained may be regarded as comparative quantitative expressions for the liability to ignition of these substances.

II. *Description of a Large Induction-coil.*
By WILLIAM SPOTTISWOODE, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ALTHOUGH I have not as yet many experimental results sufficiently complete for communication to your Magazine, I still think that the construction of an induction-coil capable of giving a spark 42 inches in length is an instrumental feat deserving of record in the annals of science. I therefore venture to submit the particulars of this coil, recently completed for me by Mr. Apps, of 433 Strand, to whose skill and perseverance the success of the undertaking is due.

The general appearance of the instrument is represented in the following figure, by which it is seen that the coil is supported by two massive pillars of wood sheathed with gutta-percha, and filled in towards their upper extremities with paraffine wax. Besides these two main supports, a third, capable of being raised or lowered by means of a screw, is placed in the centre, in order to prevent any bending of the great superincumbent mass. The whole stands on a mahogany frame resting on castors.

The coil is furnished with two primaries, either of which may be used at pleasure. Either may be replaced by the other by two men in the course of a few minutes. The one to be used for long sparks, and indeed for most experiments, has a core consisting of a bundle of iron wires each ·032 inch thick, and forming together a solid cylinder 44 inches in length and 3·5625 inches in diameter. Its weight is 67 lbs. The copper wire used in this primary is 660 yards in length, ·096 inch in diameter, has a conductivity of 93 per cent., and offers a total resistance of 2·3 ohms. It contains 1344 turns wound singly in 6 layers, has a total length of 42 inches,

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ignition of explosive mixtures of hydrocarbons and air is measured by Bunsen's method. The maximum velocity for marsh-gas and air was 0·524 millim., the minimum 0·041 millim. per second. The numbers for coal-gas and air were—maximum 1·01 millim. and minimum 0·097 millim. per second. The velocities are in these instances very slow; and the experiments show that they are still further reduced by an excess of either constituent of the mixture.

APPS'
PATENT INDUCTORIUM.
433 STRAND.
LONDON.
N°. 8477.

W. J. WELCH SC
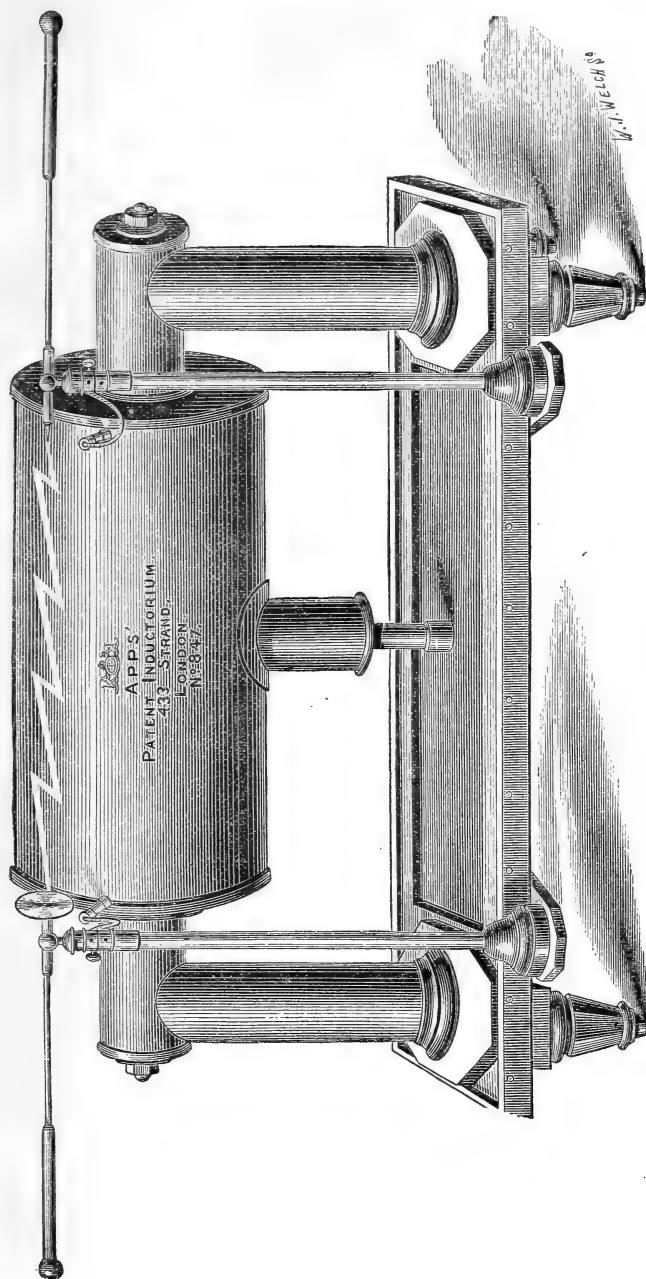
with an internal diameter of 3·75 inches and an external of 4·75 inches.   The total weight of this wire is 55 lbs.

The other primary, which is intended to be used with batteries of greater surface, *e. g.* for the production of short thick sparks, or for spectroscopic purposes, has a core of iron wires ·032 inch thick, forming a solid cylinder 44 inches long and 3·8125 in diameter.   The weight of this core is 92 lbs. The copper wire is similar to that in the primary first described; but it consists of 504 yards wound in double strand forming three pairs of layers whose resistances are ·181, ·211, ·231 ohms respectively.   Its length is 42 inches, its external diameter 5·5, and its internal 4 inches.   Its weight is 84 lbs. By a somewhat novel arrangement, these three layers may be used either in series as a wire of ·192 inch thickness, or coupled together in threes as one of ·576 inch thickness. It should, however, be added that, owing to the enormous strength of current which this is capable of carrying, and to the highly insulated secondary coil being possibly overcharged so as to fuse the wire, this larger primary is best adapted for use with secondary condensers of large surface, for spectrum-analysis, and for experiments with vacuum-tubes in which it is desirable to produce a great volume of light of high intensity as well as of long duration at a single discharge.   The alternate discharges and flaming sparks can also be best produced by this primary.   It has been used for high-tension sparks to 34 inches in air, the battery being 10 cells of Grove's with platinum plates $6\frac{1}{4} \times 3$ inches.   Great facilities for the use of different sets of batteries are afforded by the division of this primary into three separate circuits, to be used together or separately; and by a suitable arrangement of automatic contact-breakers, the primary currents may be made to follow in a certain order as to time, duration, and strength, with effects which, when observed in the revolving mirror, will doubtless lead to important results in the study of striæ in vacuum-tubes.

We now come to the secondary, which consists of no less than 280 miles of wire, forming a cylinder 37·5 inches in length, 20 inches in external, and 9·5 inches in internal diameter. Its conductivity is 94 per cent.; and its total resistance is equal to 110200 ohms.   The whole is wound in four sections, the diameter of the wire used for the two central sections being ·0095 inch, and those of the two external being ·0115 inch and ·0110 inch respectively.   The object of the increased thickness towards the extremities of the coil was to provide for the accumulated charge which that portion of the wire has to carry.

Each of these sections was wound in flat disks ; and the average number of layers in each disk is about 200, varying, however, with the different sizes of wire, &c.   The total number of turns in the secondary is 341,850.

The great length of the wire necessary can be easily understood from the fact that near the exterior diameter of the coil a single turn exceeds 5 ft. in length.   The spark, it is believed, is due to the number of turns of wire, rather than to its length, suitable insulation being preserved throughout the entire length.   In order to ensure success, the layers were carefully tested separately and then in sets, and the results noted for comparison.   In this way it was hoped that step by step safe progress would be made.   As an extreme test, as many as 70 cells of Grove's have been used, with no damage whatever to the insulation.

The condenser required for this coil proves to be much smaller than might at first have been expected.   After a variety of experiments, it appeared that the most suitable size is that usually employed, by the same maker, with a 10-inch-spark coil—viz. 126 sheets of tinfoil $18 \times 8 \cdot 25$ inches in surface, separated by two thicknesses of varnished paper, the two thicknesses measuring ·011 inch.   The whole contains 252 sheets of paper 19×9 inches in surface.   I hope, at some future opportunity, to make further experiments with other condensers.

Using the smaller primary, this coil gave, with 5 quart cells of Grove, a spark of 28 inches, with 10 similar cells one of 35 inches, and with 30 such cells one of 37·5 inches and subsequently one of 42 inches.   As these sparks were obtained without difficulty, it appears not improbable that, if the insulation of the ends of the secondary were carried further than at present, a still longer spark might be obtained.   But special adaptations would be required for such an experiment, the spark of 42 inches already so much exceeding the length of the secondary coil.

When the discharging points are placed about an inch apart, a flowing discharge is obtained both at making and at breaking the primary circuit.   The sound which accompanies this discharge implies that it is intermittent, the time- and current-spaces of which have not as yet been determined.

With a 28-inch spark, produced by 5 quart cells, a block of flint glass 3 inches in thickness was in some instances pierced, in others both pierced and fractured, the fractured pieces being invariably flint glass.   If we may estimate from this result, the 42-inch spark would be capable of piercing a block 6 inches in thickness.

When used for vacuum-tubes this coil gives illumination of extreme brilliancy and very long duration : with 20 to 30 cells and a slow-working mercury break, giving, say, 80 sparks per minute, the striæ last long enough for their forward and backward motion to be perceived directly by the unassisted eye.   The appearance of the striæ when observed in a revolving mirror (as described in the Proceedings of the Royal Society, vol. xxv. p. 73) was unprecedentedly vivid, and this even when only two or three cells were employed.

III. *On the Number o the Univalent Radicals* $C_n H_{2n+1}$.
By A. CAYLEY, *Esq.*[*]

I HAVE just remarked that the determination is contained in my paper " On the Analytical Forms called Trees " &c., British-Association Report, 1875 ; in fact, in the form $C_n H_{2n+1}$ there is one carbon atom distinguished from the others by its being combined with (instead of 4, only) 3 other atoms ; viz. these are 3 carbon atoms, 2 carbon atoms and 1 hydrogen atom, or else 1 carbon atom and 2 hydrogen atoms (CH_3, methyl, is an exception ; but here the number is $=1$). The number of carbon atoms thus combined with the first-mentioned atom is the number of main branches, which is thus $=3, 2$, or 1 ; hence we have, number of radicals $C_n H_{2n+1}$ is $=$

No. of carbon root-trees C_n with one main branch,
+ No. of   „   „   with two main branches,
+ No. of   „   „   with three main branches;

and the three terms for the values $n=1$ to 13 are given in Table VII. (pp. 296, 297) of the paper referred to.
Thus $n=5$ (an extract from the Table) is

Index x, or number of knots.	Index t, or number of main branches.	Altitude.					
		0	1	2	3	4	
5	1			1	2	1	4
	2			2	1		3
	3			1			1
	4		1				1
	Total ...		1	4	3	1	9

and the number of the radicals $C_5 H_{11}$ (isomeric amyls) is

[*] Communicated by the Author.

$4 + 3 + 1 = 8$ (or, what is the same thing, it is $9 - 1$, the corner-total less the number immediately above it). The tree forms corresponding to the numbers 1, 2, 1; 2, 1; 1 in the body of the Table are the trees 2 to 9 in the figure, p. 258.

The numbers of the radicals $C_n H_{2n+1}$, as obtained from the Table in the manner just explained, are:—

$n =$	Number of radicals $C_n H_{2n+1}$.				
1	1		$=$	1	Methyl.
2	1			1	Ethyl.
3	1			1	Propyl.
4	4			4	Butyls.
5	9	$-$	1	8	Amyls.
6	18	$-$	1	17	Hexyls.
7	42	$-$	3	39	Heptyls.
8	96	$-$	7	89	Octyls.
9	229	$-$	18	211	Nonyls.
10	549	$-$	42	507	Decyls.
11	1346	$-$	108	1238	Undecyls.
12	3326	$-$	269	3057	Dodecyls.
13	8329	$-$	691	7638	Tridecyls.

The question next in order, that of the determination of the number of the bivalent radicals $C_n H_{2n}$, might be solved without much difficulty.

Cambridge, November 20, 1876.

IV. *Note on the Action of the Flame of Alcohol upon the Metal Palladium.* By Professor Wöhler, *For. Mem. R.S. &c.*[*]

IN the year 1824 I published[†] the observation that palladium, both in the spongy form and in that of foil, possesses the property of becoming gradually covered with a thick coating of carbon when held in the flame of a spirit-lamp. A small piece of palladium sponge thus heated swells up to many times its own volume, cauliflower-like bunches of carbon being deposited on the surface of the metal. The same phenomenon is observed if the metal be allowed to glow in a coal-gas flame. When the adhering porous mass of carbon is allowed to burn away, a fine skeleton of palladium remains behind; and this is the case even if the carbon has been deposited upon a piece

[*] Translated by Professor Roscoe from the *Göttinger Nachrichten*, No. 20, 1876.
[†] Pogg. *Ann.* vol. iii. p. 71.

of foil, which is then found to have been penetrated through and through with carbon and rendered quite brittle.   At the time of that publication I believed that a peculiar affinity must be supposed to exist between palladium and carbon; but the remarkable discovery by Graham of the occlusion of hydrogen by palladium makes it probable that the above phenomenon is rather connected with this power of the metal to absorb many hundred times its volume of hydrogen.

The behaviour of palladium to olefiant gas appeared likely to throw light upon this question.   More than 6 grams of chemically pure spongy palladium, which, as experiment had shown, was capable of absorbing many hundred times its volume of hydrogen, was placed in a tube, and a slow current of ethylene led for some hours over the metal heated to 100°. Neither at this temperature nor at other higher temperatures below a red heat did any action take place; no gas was absorbed, no blackening of the metal could be observed.   Only when the glass tube in which the metal was placed was heated to redness was carbon deposited with evolution of hydrogen. But the point at which the decomposition of ethylene began in presence of the metal was found to be lower than that necessary to bring about decomposition of the gas in the absence of palladium.   Marsh-gas was also found to be without any action on palladium.

From the above experiment, it appears that palladium is unable to absorb ethylene or the gases of the alcohol-flame, but that it is able to bring about the deposition of carbon from such gases at a temperature lower than that at which ethylene by itself undergoes decomposition.

Possibly this action may be explained by the supposition that a combination of hydrogen goes on contemporaneously with the deposition of carbon, but that at the same time a rapid dissociation of the hydrogen compound occurs—much in the same way as we may explain the remarkable physical changes which take place in copper when heated in ammonia-gas by a temporary combination of the nitrogen.   A circumstance which favours this explanation is, that the bright palladium-foil, after it has been exposed to the action of the alcohol- or ethylene-flame, has altogether lost its brilliancy as well as its malleability.

The fact that when palladium is fused with carbon it does not take up any of this element, is a sufficient proof that the phenomenon in question is not caused by the affinity of the metal for carbon ; and this fact was proved in 1857 by Dr. Th. Wood, who at my suggestion made an examination on the relations of palladium to carbon compounds.   His experiments were afterwards carried out in Professor Bunsen's

laboratory*; but he was unable to obtain satisfactory results so far as the main question is concerned.

The apparatus shown in the figure serves as a simple and effective means of exhibiting the absorption of hydrogen by palladium. *a* is a tall glass cylinder filled with water; *b* is a
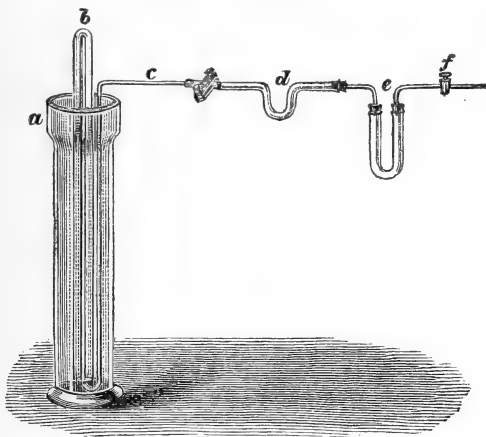
divided tube of at least 100 cubic centims. capacity; *c* is a very narrow gas-delivery tube, one end of which passes under the open end of the divided tube; *d* is a tube for the palladium, bent so that it can be placed in a vessel of boiling water; *e* is a chloride-of-calcium tube for drying the hydrogen; *f* a glass stopcock leading to a gas-holder.

After a few grams of palladium sponge have been placed in the tube *d*, whilst the one end of the tube remains open a current of hydrogen gas is passed for some time over the metal placed in the boiling water. In the mean time the tube *b* is filled with water, and the cylinder as well as the tube *c*. After the lapse of about half an hour the tube *d* is taken out of the hot water and allowed to cool, the current of gas still passing through. Then the stopcock *f* is closed, the drawn-out end of the tube *d* connected with the gas-delivery tube *c* and the screw-tap loosed.

The palladium is now heated; and the occluded gas is quickly set free and passes into the graduated tube *b*. As the metal cools, the gas is again absorbed; and at last all the hydrogen disappears and the tube *b* becomes, as before, full of water. An apparatus of this kind, fitted with glass stopcocks, may therefore serve for exhibiting the experiment any wished-for

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* Th. Wood, 'The Action of Palladium on Carbon:' Göttingen, 1859.

number of times. If the palladium sponge, when saturated with hydrogen, be brought into the air, it becomes red hot. Palladium which has become of a bluish-green tint from ignition in the air, becomes hot when plunged into hydrogen and assumes the original grey colour of the metal.

V. *Reports from the Chemical Laboratory of Trinity College, Dublin. By* J. EMERSON-REYNOLDS, *M.D., M.R.I.A., Professor of Chemistry, University of Dublin**.

No. 1.—*On Glucinum: its Atomic Weight and Specific Heat.*

AMONGST the few rare elements found in Ireland is the metal glucinum or beryllium, which occurs in the well-known alumino-glucinic silicate, beryl or "emerald." This mineral is found in comparative abundance, though in a rough state, in the granites of Donegal, and is somewhat less freely distributed through the granites of the Mourne Mountains in the county of Down. As the "atomic weight" of glucinum has not yet been definitely fixed by the determination of the specific heat of the metal, it seemed desirable that we in Ireland should make the necessary crucial experiments. Hence, about seven years ago, I commenced to collect the crude Irish beryls or "emeralds," and ultimately succeeded in obtaining 3 kilogrammes of the dressed mineral, from which I prepared nearly 350 grammes of the pure glucinic oxide.

I have to thank my friend Mr. William Harte, C.E., the excellent County Surveyor of Donegal, for the valuable assistance he kindly afforded me in collecting much of the mineral from which the glucinic oxide was prepared.

The satisfactory nature of the results of a set of preliminary experiments with the material at my disposal must be my apology for laying a short communication upon the subject before the Academy at a very early stage of the investigation.

Some glucinic oxide was converted into the anhydrous chloride by the action of chlorine upon it at a full red heat in presence of finely divided carbon; and the metal was subsequently procured by the action of metallic sodium on the pure sublimed glucinic chloride. The reduction was effected by heating a suitable mixture in a platinum vessel; but the temperature was not allowed to rise sufficiently to liquefy the mass; and on removal of the material from the crucible, those portions which had been in contact with the platinum were rejected. The resulting mixture of sodic chloride and reduced glucinum

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* Communicated by the Author, having been read before the Royal Irish Academy, April 10, 1876.

was then fused under common salt in a lime crucible; this precaution was taken in order to avoid contact with siliceous compounds. Considerable loss occurred in this operation; but I succeeded in obtaining a small coherent mass of metallic glucinum, which latter was found to agree in characters with the metal described by Debray[*], though that distinguished chemist effected the reduction of his metal in a different manner.

If we admit, with Awdejew and with Debray, the number 4·6 to be the equivalent of glucinum (H=1), the question remains whether the atomic weight, so called, is a multiple of the equivalent by 2 or 3.

If, as some assert, the atomic weight is $4·6 \times 3 = 13·8$, the only known oxide of glucinum must resemble alumina. If, on the other hand, the atomic weight is $4·6 \times 2 = 9·2$, glucina must be an oxide like that of zinc or of magnesium. Each view has received the support of a group of chemists of the highest eminence; but, owing to peculiar difficulties surrounding the case, an appeal to chemical criteria has hitherto been insufficient to decide between the two conflicting opinions—a determination of the specific heat of the metal, or of the vapour-density of one of its compounds of simple constitution, being necessary for the final settlement of the question. Of these methods I chose the former; and having made several determinations of the capacity for heat of metallic glucinum, I have the gratification to state that the data obtained lead to the conclusion that the atomic weight of glucinum is double the equivalent weight. Glucinum is therefore a diatomic metal with an atomic weight of 9·2—though, I may add, this number may be slightly affected by a new determination of the equivalent, in which I am engaged.

The method pursued in making the necessary determinations upon which to found the conclusion just stated was devised for the purpose of this inquiry; and as it is essentially different from any with which I am acquainted, I may be permitted to indicate very briefly the plan adopted after a good deal of preliminary investigation[†].

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[*] *Annales de Chimie et de Physique,* troisième série, tom. xliv. p. 5 (1855).

[†] The preparation of pure metallic glucinum in quantities exceeding two or three grammes is difficult and costly. For this amongst other reasons I determined to employ Bunsen's admirable and theoretically perfect ice-calorimeter in the estimation of the specific heat of the metal, as small quantities of material only are required. It proved, however, to be impossible, owing to various engagements, to prepare the glucinum in a state of sufficient purity until the season had passed when Bunsen's ice-calorimeter can be conveniently used. I had therefore to devise a calorimetric method which could be employed during the warm weather, and which could

The well-known law of Dulong and Petit, as modified by Cannizaro, asserts that the atoms of elementary matter have the same capacity for heat, when we compare them in the solid state. The outstanding exceptions to this important law are few; and even these appear to have been cleared away in some degree by the recent researches of Weber on the specific heats of silicon, boron, and carbon. The principle, however, is admittedly sufficiently general in its application to enable us to found upon it a plan for the determination of the atomic weight, so called, of a particular element; for it is evident that if we employ as a standard a metal whose atomic weight and specific heat are both accurately known—silver for example ($=100$)—the weight of another solid element which contains the same quantity of heat at 100° C. as 108 parts of pure silver at 100° C. is the atomic weight of the element. In seeking to compare glucinum with pure metallic silver in this way, I succeeded in arranging an experimental method which not only enabled me to attain the object I had in view, but also to demonstrate the truth of the law just referred to. The apparatus required is easily constructed, and consists of a spirit-thermometer with a cylindrical "bulb" in which a test-tube is sealed, after the manner of Bunsen's ice-calorimeter. This part of
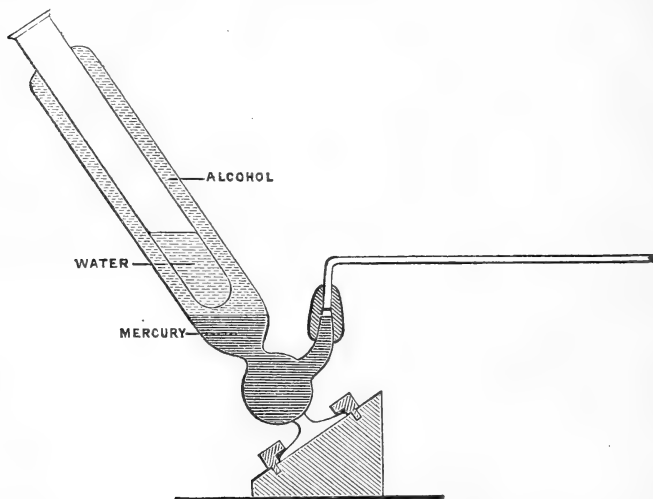
the apparatus can be conveniently made from a small chloride-

afford trustworthy results with small weights of material. I have given in the text an outline of this method; but the details of its application to the determination of atomic and molecular heat will form the subject of another communication.

of-calcium drying-tower, as shown in the diagram. Although the larger "bulb" of the thermometer is full of spirit, the lower one and the stem are full of mercury, and connected with a fine capillary tube carefully graduated in millimetres, and calibrated. The arrangement constitutes an exceedingly delicate spirit-thermometer, with a mercury index.

When it is desired to compare a solid element with silver, in order to fix the atomic weight, it is necessary to make a preliminary experiment with the standard metal. For this purpose one cubic centimetre of distilled water is placed in the test-tube, which is immersed in the bulb of the thermometer; and when the temperature has been equalized, and the thread of mercury has reached a suitable position in the stem, a piece of pure silver weighing 108 centigrammes, and heated to 100° C. in steam, is rapidly dropped into the cubic centimetre of water, and the expansion caused in a given time carefully noted*. According to the law above stated, a centigramme atom, if I may use the term, of any other metal than silver, ought to cause exactly the same expansion when the experiment is made with it under precisely the same conditions; and these conditions are very easily realized. I have ascertained that such is the case; and the approximate equality in "atomic heat" of many of the metals has thus been easily demonstrated.

The comparison of glucinum with silver was made on this plan; and it was found that the weight of glucinum which contains nearly the same quantity of heat at 100° C. as 108 centigrammes of silver at the same temperature is not 4·6 or 4·6 × 3, but 4·6 × 2, or 9·2 centigrammes.

The "atomic heat" of silver, or the product of the specific heat ($= ·05701$ according to Regnault) into the atomic weight ($= 108$), is 6·157. Using this number as the standard for reference, the experimental number found for the atomic heat of the specimen of glucinum operated with is 5·91. Thus:—

$$\text{Atomic heat of silver} \quad . \quad . \quad = 6·157$$
$$\text{Atomic heat of glucinum} \quad . \quad = 5·910$$

The difference is less than the known difference between the atomic heat of silver and that of aluminum; but I am inclined to think that the lower number found for the glucinum used is due to the presence of a little platinum in the specimen of metal. Owing to the high atomic weight of platinum ($= 197·1$) as compared with that of glucinum (9·2), the presence of even a small quantity of the former metal must very sensibly affect the determination of the atomic heat of glucinum. I hope

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* The apparatus is carefully protected from the influence of air-currents during an experiment.

soon to be in a position to continue these experiments with the *pure* metal.

It will, however, appear from the following considerations that we may fairly regard the above determination of the atomic heat of glucinum as being of such value as to enable us, even at an early stage of the inquiry, to use it as a physical control, and to fix the atomic weight of the metal, subject of course to the probably small change in the numerical expression which may prove to be necessary as the investigation proceeds.

If we assume the atomic weight of glucinum to be 9·2, and employ the value I have obtained for the atomic heat, *i. e.* 5·91, we can calculate the specific heat of the metal by means of the formula

$$S = \frac{H}{A}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

when S represents the specific heat, H the atomic heat, and A the atomic weight of an element. The specific heat of glucinum thus calculated is ·642.

If now we substitute for H a constant, which in this case is the product of the well-ascertained atomic weight of silver[*] into its equally well-determined specific heat, AS = 6·157, the expression becomes

$$S = \frac{6 \cdot 157}{A}; \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and with its aid we can calculate the specific heat of any solid element, if its atomic weight is known or assumed. I have thus calculated the specific heat of glucinum on the assumption (*a*) that its atomic weight is 9·2, (*b*) that its atomic weight is 4·6, and (*c*) that it is 13·8.

The results are compared in the following Table with the specific heat obtained by calculation from the actual determination of the atomic heat of the metal:—

Specific heat of glucinum calculated (1) from the result of determination of atomic heat.

A = 9·2.   .   .   .   .   .   ·642.

Specific heat of glucinum calculated by (2).

When A = 9·2   .   .   ·669
When A = 4·6   .   .   1·338
When A = 13·8   .   .   ·446.

I am therefore justified in concluding that the atomic weight of glucinum is nearly if not exactly 9·2.

[*] We might obviously take any other product; but that of silver is here preferred because the atomic heat of that metal has been employed as the standard for reference.

VI. *On a permanent Deflection of the Galvanometer-needle under the influence of a rapid series of equal and opposite induced Currents.* By Lord Rayleigh, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE publication, in your December Number, of a memoir by Mr. Chrystal on Bi- and Unilateral Galvanometer Deflection recalled to my mind some observations of a like character made some years ago by myself. I have lately succeeded in finding the manuscript of a communication with the above title read (literally) before the British Association at Norwich in 1868, which contains a short account of these observations. As the subject has acquired an additional interest in consequence of the investigations of Dr. Schuster and Mr. Chrystal, I shall be glad if you can find room for my paper, which has not been printed in full hitherto.

<div align="center">I am, Gentlemen,</div>

<div align="right">Your obedient Servant,</div>

<div align="right">Rayleigh.</div>

Terling Place, Witham,
December 11, 1876.

The following paper contains a short account of some experiments which led to rather unexpected results, of which I can find no notice in the methodical treatises on Electricity, although they might seem to be in the way of any experimenter on induced currents. The arrangement of the first experiment was nearly the same as that described by Faraday in his original memoir on induction. Two thick copper wires were coiled together—the circuit of one being completed by the battery and make-and-break apparatus, and that of the other by an ordinary astatic galvanometer of moderate sensitiveness. The make-and-break arrangement was a very rude one of my own construction, acting either by the dipping of needles into mercury, or by the intermittent contact of a spring with a toothed wheel. When the handle of the instrument is turned, there are generated in the second circuit, as is well known, a series of instantaneous currents which are alternately opposite in sign but whose magnitudes are equal, although that corresponding to the break of the battery-circuit is the most condensed. When, then, the instrument is worked with such rapidity that the interval between the currents is very small in comparison with the time of free oscillation of the needle, the latter might be expected to be sensibly unaffected. But so far was this from being the case, that although the swing of

the needle produced by a single impulse was only a few degrees, yet under the influence of the series of equal and opposite currents it remained steady at 60 or 70, and *that* on either side of the zero-point, which had in fact become a position of unstable equilibrium.   Since it took place indifferently in either direction, the deflection cannot be ascribed to an inequality in the alternate currents, giving on the whole a balance in one direction such as, according to the experiments of Henry d'Abria, might arise from imperfect contacts in the second circuit.

The first explanation which suggested itself to me was that, while no doubt the currents of the two series were strictly equal (numerically), the resulting impulses, or rather impulsive couples, acting on the needle might be slightly different owing to the change of the latter's position in reference to the coil in the small vibration which the series of currents must produce, however quickly they may follow one another, which would give one set an advantage over the other.   Those currents would prevail which tend to increase the deviation of the needle ; for they would have, as it were, the greatest purchase on it.   To make this perfectly clear, suppose either the galvanometer to be turned round, or the direction of the magnetic force altered by permanent magnets, so that the position of equilibrium of the needle is now no longer zero, but say 20°, and then let the series of induced currents pass.   There might appear at first sight to be two cases, according as the first current tends to diminish or increase the already existing deviation; but the result is the same in both, and I will take for the sake of illustration that in which the needle is first sent towards zero.   When the second instantaneous current passes, it finds the needle nearer zero, and therefore acts upon it with greater force than did the first ; and this process continues, so that if for the moment we imagine the needle to vibrate about 20°, there is an outstanding force tending to increase the deviation. As this is unbalanced, the equilibrium at 20° cannot be maintained, and the needle must move further from zero: instead of equilibrium, perhaps, I should say resultant equilibrium ; for the rapid vibration of the needle just now referred to of course goes on in any case.   I worked out the mathematical theory of this action fully for a tangent-galvanometer ; and for the case, to which experiment is not limited, of an equal interval between consecutive instantaneous currents of opposite sorts. The most conspicuous result (which might, however, have been anticipated) was that the effect is independent of the rapidity with which the make-and-break apparatus works.   As this was not at all what I had inferred from the experiment, I began to

doubt whether I had hit upon the true cause of the phenomenon ; and on more close examination of the mathematical result, it appeared that the needle could not remain permanently deflected from its position of equilibrium at zero, unless each instantaneous current was powerful enough to swing it right round when acting on it alone, although an already existing deviation would be always increased. I have already mentioned that the phenomenon was observed when the swing for a single current was only a few degrees, so that there is no doubt of the inadequacy of the foregoing explanation.

The real cause is, I believe, to be found in a deficiency in the hardness of the steel needles, rendering them to some extent capable of temporary magnetism when placed in a field of force. If this temporary magnetism alone be considered, the two sets of instantaneous currents conspire in their effects instead of opposing each other ; for if a soft-iron needle be freely suspended in a uniform field of magnetic force, it has, as is known, four positions of equilibrium, of which those two are stable which would be positions of equilibrium (one stable and one unstable) for a magnetized steel bar. If while the needle is in equilibrium the direction of the magnetic force is reversed, no disturbance takes place, because the magnetism of the needle is at the same time reversed also. If such a needle be suspended in the coil of a galvanometer, the force with which a current acts upon it is independent of the direction and varies as the square of the current ; or when there is a rapid series of varying but periodic currents, the deflecting force varies as the integral of the square of the current, and as the sine of twice the deviation from zero. The deflecting force would, according to this, be for a given position of the needle with reference to the coil (or deviation) proportional to the heating-power of the discontinuous current ; but it must be remembered that the case is an ideal one, as no iron is perfectly soft or capable of at once assuming the magnetism due to the field of force in which it is placed. A remarkable illustration of this will be mentioned a little later.

In order to test the correctness of these views, I removed the steel needles from the galvanometer and replaced them by a single soft-iron needle, with which it was found that all the phenomena observed before were reproduced. Being anxious to submit the arrangement to a more severe test, I placed the galvanometer in a third circuit, so that it should be acted on by the currents induced by the induced currents of the second circuit, as in Henry's experiments. The effect was very marked, though for this it was necessary that the galvanometer should be turned round so that the position of equilibrium should be

about 20° or 25°; in a tangent-galvanometer the most favourable position would be at 45°. Throughout these experiments the effect always increased with the velocity of the contact-breaker up to a certain point, about 100 per second, and then declined. The general increase is in accordance with the explanation here advanced, while the falling off might be owing to an imperfect action of the make-and-break machine when a certain velocity is reached. I am more inclined, however, to attribute it to a want of theoretical softness in the iron, which prevents it from taking the full magnetism when the alternation of currents is too rapid. In support of this opinion I adduce one more experiment. Returning to the first arrangement, in which the galvanometer was placed in the second circuit, I arranged a third circuit in the neighbourhood of the second or galvanometer circuit, whose ends could be joined or kept apart *. In the second case, of course, no effect is produced by the third circuit ; but what will be the result of completing it ? It is known that while the magnitude of each instantaneous current in the galvanometer circuit is unaffected, the duration of them is increased by the induction. This dilution, so to speak, of the induced currents diminishes their heating-power, which depends on the integral of the square of the current while it lasts, and would, if the iron were perfectly soft, diminish the deflecting force on the galvanometer-needle ; but it was found, on the contrary, considerably to increase it. In fact, the induced currents are too condensed to produce their full action, passing away before the needle is properly magnetized.

It is too soon to say whether any use can be made of these results ; but it is possible that such a soft-iron galvanometer might be available for measuring the currents produced by the new magneto-electric machines when the consecutive waves are opposite.

VII. *On the Alkaline Development of the Photographic Image.*
By Captain Abney, *R.E., F.R.S.*†

SINCE alkaline solutions have been introduced for the development of the photographic image, there has been a certain amount of ambiguity regarding their action. It has

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* There were two similar coils, each containing two wires A_1, A_2, B_1, B_2. The battery-circuit included A_1 and the interrupter. The second circuit consisted of the wires A_2, B, and the galvanometer. B_2 gave the third circuit, which was closed or open, according as the ends of B_2 were in connexion or not.— *Note added* Dec. 1876.

† Communicated by the Author.

usually been assumed that their sole function is to reduce to the metallic state the particles of silver bromide which have been acted upon by light.

The alkaline developer consists of pyrogallic acid or other oxygen-absorber, an alkali such as ammonium hydrate, and a restrainer such as potassium bromide. These are generally mixed together and applied to the film on which has been impressed an invisible image in the camera. Those parts acted upon by light darken under the influence of the solution; whilst, if the surface be in a proper condition and the proportion of the restrainer to the alkali be well balanced, the portions unacted upon by light remain unchanged. The image thus formed is soluble in nitric acid; and further tests show it to be metallic silver. In order to discover the part which the alkaline developer played to cause this reduction, a large series of experiments have been conducted in the laboratory of which I have charge; and the results appear sufficiently interesting to be published.

Bulbs were made of the shape shown in the accompanying figure. In A was placed, whilst carefully excluded from all light, thoroughly washed silver bromide and pyrogallic acid. In B was placed a solution of potassium hydroxide or other alkali except ammonium hydrate. If ammonium hydrate were required, the bulb B was made double; in the first was placed ammonium nitrate and in the other potassium hydroxide, calculated so that the ammonium salt should always be in excess when
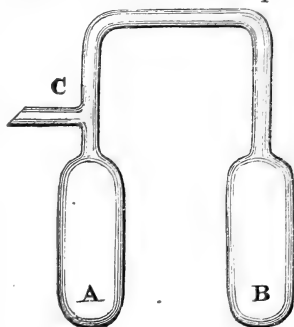
the potassium was brought into contact with it. The tube C was attached to a Sprengel pump; and when the apparatus was exhausted, C was sealed, and the whole of the alkali caused to enter A. After from 2 to 60 hours the bulbs were broken, and the solids and liquid kept for analysis. As far as can be judged, the silver bromide was instantaneously attacked by the alkaline pyrogallate. (There is no action due to pyrogallic acid on the silver bromide without the presence of an alkali, as was proved by keeping them in contact for weeks and noting their appearance.) After a large number of experiments, it was found that the amount of silver bromide capable of reduction was primarily dependent on the amount of pyrogallic acid present, and in a secondary degree on the amount of alkali present. Thus 1 equivalent of pyrogallic acid can

reduce 4 equivalents of silver bromide to the metallic state if sufficient alkali be present to combine with the bromine liberated. The solution, after evaporation to dryness, was found insoluble in benzole or ether &c., and only partially soluble in alcohol. A part of that dissolved was further found to be the bromide of the alkali; and the excess of alkali not in combination with carbonic acid was also found in the solution. The amount of carbonic acid was found to be considerable, equal to 1 equivalent of pyrogallic acid. (It seems unnecessary to give the analysis of the organic residue. Apparently the compound formed is totally different from that obtained by Stenhouse[*] when investigating the action of bromine on alkaline pyrogallate.) From the above experiment, it will be noted that the reduction of the silver is independent of the absorption of external oxygen, and that only a definite amount of silver can be reduced by a given amount of pyrogallic acid.

When, in addition to the alkali, a large excess of soluble bromide was added to the bulb B, the same results were obtained, though the reduction of the silver bromide seemed to take rather longer time to effect. The following is an example of the quantities employed in these experiments:—

In bulb A was placed . . 300 grains of AgBr and
                                 10 grains of $C_6 H_6 O_3$;
In the bulb B was placed . 300 grains of KHO

dissolved in the least possible quantity of water.

The amount of Ag found to be reduced was 33·86 grains; and the amount of KBr found in solution was 36·97 grains.

Layers of thoroughly exposed and unexposed silver bromide were next treated with a rather weaker solution of the alkaline developer; and when the restraining soluble bromide was omitted, the reduction to the metallic state took place in each layer almost equably. When the soluble bromide was present in equivalent quantities with the alkali, the reduction took place first in the layer that had been exposed to light, and spread gradually into the other layer. From this we may gather that the exposed silver bromide is more readily reduced than that unexposed, and that the solution of soluble bromide and the alkali acts less vigorously than the alkali alone.

Another point to investigate was as to the means by which the density in an alkaline-developed image was produced; for it could not be supposed that merely those atoms of silver bromide which had been reduced to the state of subbromide would be attacked, since the subbromide of silver is essentially a coloured compound and can be distinguished even in small

[*] Journal of the Chemical Society, January 1875.

quantities. A plate prepared with silver iodide was flowed over with tannin and dilute albumen, and dried. It was then exposed in the camera, and after exposure half of it coated with an emulsion formed in collodion by silver bromide. Now a photographic image impressed on silver iodide is not amenable to alkaline development unless the solutions be excessively strong. An alkaline developer made as below was therefore employed :—

1. Pyrogallic acid . . 16 grains.
   Water . . . . . 1 oz.

2. Potassium bromide . 20 grains.
   Water . . . . . 1 oz.

3. Liquor ammoniæ ($\cdot$880) $\frac{1}{2}$ oz.
   Water . . . . . 8 oz.

or   Potassium hydroxide. 15 grains.
   Water . . . . . 1 oz.

One part of Nos. 1 and 3 were added to every three parts of No. 2. As might have been expected, but a trace of an image was seen on applying the developing solutions to the uncoated iodized plate (and this trace was subsequently proved to be due to the sensitive albumen salt); but where the emulsion had been used, an image gradually appeared, not very strong, but still perfectly visible and of printing density.

Silver bromiodide plates were treated in the same way: in this case there was a feeble image on the part uncoated with bromide emulsion; but on that part coated with emulsion the image appeared on the bottom surface of the emulsion film, and gradually worked its way up till reduced silver was obtained on the top surface, where the light had most strongly acted on the exposed film. The recoated half-plate, on fixing with potassium cyanide, gave a perfectly bright image, clear and dense, whilst on the other half it remained feeble.

With bromide plates in which only a feeble image could be obtained, the same procedure gave the requisite density; and this fact is likely to be of practical value.

It was next proposed to ascertain in which film the developed image was really situated—whether in the exposed or the unexposed film. The double films, which had been treated as described, were taken from the glass plate by applying a damped piece of gelatinized paper to the surface; and after detachment, a similar piece of paper was applied to the surface which had been next the glass. When very nearly dry, the two pieces of paper were pulled apart; one film was found attached to one and the other to the other. Considerable difficulty was found in this operation, and only about 20 per

cent. of the whole were properly manipulated, through our not being always able to hit on the exact amount of desiccation.

In examining these films differences were observable.   In some cases the image was found to lie almost entirely in the exposed film, whilst in others a strong image was obtained in the unexposed film.   The difference was eventually found to depend on the alkali used in the developer, and on the porosity of the collodion in which the emulsion was formed.   When ammonium hydrate was employed and a porous emulsion, the image was on the exposed film ; whilst if potassium hydroxide were employed, a vigorous image was in the top unexposed film.   This can be explained through the solubility of silver bromide in ammonium hydrate.   The silver bromide would be first dissolved by the ammonium, and then during the course of reduction be carried down to the reduced silver beneath it.   Since silver bromide is insoluble in potassium hydroxide, the haloid would be reduced *in situ*.

Having determined this point, it was next endeavoured to ascertain if the photographic image impressed by light on the bottom surface caused a sympathetic action in the unexposed emulsion film.   Plates were prepared as before, one half being coated with emulsion after exposure, and put aside for some days.   The films were then separated as described, and to each the developer was applied separately.   With one or two exceptions, *no image was obtained on the unexposed film*.   The reason of an image (always imperfect) being obtained in some cases was traced to the adhesion to it of the thin layer of albumen, with which the exposed film was coated ; for when the albumen was applied after exposure, no image was developed.

The foregoing experiments clearly show that the photographic image has no power of transferring itself, or of creating a sympathetic action in an unexposed film *previous to development*, and that therefore the increase of density and formation of a secondary image must be due to other action than chemical.

If further proof be required, it is only necessary to expose a dry plate, and develop it in the ordinary manner, and after drying to coat it with an emulsion, and develop again.   It will be found that where the metallic silver of the first image is beneath it, there the top film develops and gives a counterpart of it.   The metal exercises an attraction for the silver on the point of being reduced from the bromide in a similar way that it does when a silver tree is built up, or when a developed image is intensified by the application of pyrogallic acid and silver nitrate.   There seems to be a

further action, however, which must be taken into account. It has already been shown that silver subbromide is more easily reduced to the metallic state than is the bromide. In these last experiments I have found that there is great difficulty in starting the development in the unexposed bromide film if the layer of albumen be too thick, but that when once started it proceeds more rapidly. This can be accounted for on the supposition that the attracting particles of silver were too far distant from the emulsion film to exercise their attractive power.

But it seems, from other experiments which are still in progress, that the atom of reduced silver immediately combines with the nearest molecule of silver bromide and forms subbromide, which is reduced to the metallic state, and its two atoms of silver combine with two other molecules of silver bromide, and so on, the image being gradually built up in this manner. Workers accustomed to alkaline development must have noticed that the feeble image first formed in the film grows in intensity rapidly up to a certain point and then flags: the reason of this is apparent if we consider the above reaction to take place, and the subsequent exhaustion of available silver bromide which can be acted upon.

One more experiment must be noted. If the film of albumen or gum &c. between the two films be very thick, a reversed action will take place, which can be explained by the fact that the strength of the developing agent is exhausted in producing the image on the exposed parts of the under film, whilst on the other portions the silver bromide most readily attackable is in the upper film.

I have already indicated that this application of a film of silver bromide after exposure, either before or after development, might prove useful when employing plates which only yield a thin image when developed in the usual manner: other applications will suggest themselves. I have also previously pointed out[*] that intensity may be given to an image by alkaline development if a silver compound soluble in the alkali be gradually added to the developer. This new method, however, seems the preferable one to adopt.

[*] Photographic News, March 27, 1874.

VIII. *On Ludlamite, a new Cornish Mineral.*
By FREDERICK FIELD, *F.R.S.*[*]

LUDLAMITE is found associated with quartz, chalybite, Vivianite, iron pyrites, and mispickel. In the gangue of some specimens galena, blende, and fluor have also been noticed.

Hardness 3·4. Specific gravity 3·12. Colour clear green, from pale to dark, transparent and brilliant. Streak very pale green, approaching white ; powder greyish white. Before the blowpipe on charcoal, tinges the flame slightly green, and yields a semifused blackish residue.

In closed tubes yields water on heating, decrepitating violently, breaking up into brilliant crystalline plates of an intense bluish-green colour by transmitted light. Soluble in dilute hydrochloric and sulphuric acids ; oxidized and dissolved by nitric acid. Perfectly insoluble in glacial acetic acid. Decomposed immediately by boiling with solution of potassium or sodium hydrate into ferrous oxide and phosphate of the alkali metal. Consists entirely, when pure, of ferrous oxide, phosphoric acid, and water. Oxidizes slightly, like Vivianite, by long exposure to air, with the formation of ferroso-ferric phosphate. The crystals contain generally minute particles of chalybite, which are very difficult to separate, also specks of iron pyrites.

Estimation of Water.—As the mineral, when heated in air, is more or less oxidized, and that very readily, it was evidently necessary to prevent oxidation. The crushed crystals were pressed between folds of warm bibulous paper to remove any small amount of mechanical water, wrapped in thin platinum-foil, and heated to low redness in an atmosphere of carbonic acid.

0·214 grm. lost 0·036 = 16·82 per cent.
0·192   „   0·033 = 17·18   „
0.230   „   0·039 = 16·95   „

Mean of analyses ... 16·98 per cent. H_2O.

Estimation of Iron.—The iron was estimated by a standard solution of potassium permanganate. When the crystals are *pure*, there is no difference in the quantity of the test-solution employed, whether before or after the addition of a deoxidizing reagent to the solution of the mineral in dilute hydrochloric acid, showing the absence of the higher iron oxides.

[*] Communicated by the Crystallological Society, having been read December 15, 1876.

Mineral.   Per cent.   Per cent. FeO.
0·196 gave 40·80 iron $=52\cdot45$
0·184 ,,   41·24 ,, $=53\cdot02$
0·243 ,,   41·13 ,, $=52\cdot85$
0·201 ,.   40·95 ,, $=52\cdot64$

Mean of iron.................... 41·03
Mean of ferrous oxide .... 52·76.

Estimation of Phosphoric Acid.—The phosphoric acid was estimated as magnesium pyrophosphate.

Mineral. $Mg_2''P_2O_7$.   Per cent. P_2O_5.
0·214 gave 0·101 $=$ 30·13
0·236 ,,   0·111 $=$ 30·09

Mean of analyses ...... 30·11.

This gives a formula closely corresponding to

$$7\,\mathrm{FeO},\quad 2\,\mathrm{P_2O_5},\quad 9\,\mathrm{H_2O}^*,$$
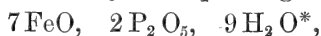
which requires the following numbers:—

	Calculated.	Found.
$7\,\mathrm{FeO}$	53·05	52·76
$2\,\mathrm{P_2O_5}$	29·88	30·11
$9\,\mathrm{H_2O}$	17·05	16·98
	99·98	99·85

That different specimens of the mineral vary somewhat in their composition there can be little doubt. Owing to the kindness of Mr. Ludlam and Mr. Talling, I had sufficient for many other analyses, not published here ; and in one or two

---

* Ludlamite is doubtless a basic ferrous phosphate ; and its relation to Vivianite or normal ferrous phosphate may with much probability be represented by the following formulæ :—

$$
\begin{array}{l}
\mathrm{PO} \begin{Bmatrix} \mathrm{O} \\ \mathrm{O} \end{Bmatrix} \mathrm{Fe} \\[2pt]
\phantom{\mathrm{PO}}\begin{Bmatrix} \mathrm{O} \end{Bmatrix} \mathrm{Fe} \\[2pt]
\mathrm{PO} \begin{Bmatrix} \mathrm{O} \\ \mathrm{O} \end{Bmatrix} \mathrm{Fe} \\
\qquad \text{Vivianite.}
\end{array}
\qquad
\begin{array}{l}
\mathrm{PO} \begin{Bmatrix} \mathrm{O} \\ \mathrm{O} \end{Bmatrix} \mathrm{Fe} \\[2pt]
\phantom{\mathrm{PO}}\begin{Bmatrix} \mathrm{O} \end{Bmatrix} \mathrm{Fe} \\[2pt]
\mathrm{PO} \begin{Bmatrix} \mathrm{O} \end{Bmatrix} (\mathrm{FeOH}) + 8\,\mathrm{H_2O} \\[2pt]
\phantom{\mathrm{PO}}\begin{Bmatrix} \mathrm{O} \end{Bmatrix} \mathrm{Fe} \\[2pt]
\mathrm{PO} \begin{Bmatrix} \mathrm{O} \end{Bmatrix} (\mathrm{FeOH}) \\[2pt]
\phantom{\mathrm{PO}}\begin{Bmatrix} \mathrm{O} \end{Bmatrix} \mathrm{Fe} \\[2pt]
\mathrm{PO} \begin{Bmatrix} \mathrm{O} \\ \mathrm{O} \end{Bmatrix} \mathrm{Fe} \\
\qquad \text{Ludlamite.}
\end{array}
$$
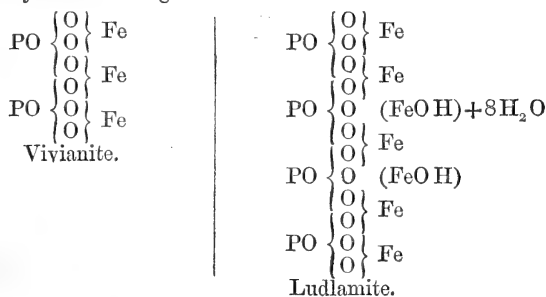
It may be regarded, in fact, as formed by the addition of a single molecule of ferrous hydrate, $Fe(OH)_2$, to two molecules of Vivianite, this addition being accompanied by the separation of a part of the water of crystallization of the latter.

instances the amount of iron was a per cent. or two under the mean of the numbers given above, as estimated by potassium permanganate. In a preliminary note sent to the ‘Chemical News’ on this mineral, it was suggested that its probable composition might be represented by the formula

$$3\,FeO,\ P_2\,O_5,\ 4\,H_2\,O\ ;$$

and in this case the amount of water is 16·7 per cent., within 0·3 per cent. of that in the formula I have adopted ; but the ferrous oxide is lower, and the phosphoric acid higher, than subsequent analyses yielded. In more than ten estimations of iron made from the clear green crystals of the minerals, 40·5 per cent. iron was the lowest number obtained. In the analyses of Vivianite, it is well known that the results of chemists have varied much as to the amount of water in the mineral. Vogel, for instance, gives 31·0 per cent., Stromeyer 27·48, and Brandes 25·00 only. Messrs. Maskelyne and Flight found 28·26, which agrees best with the recognized formula

$$3\,FeO,\ P_2\,O_5,\ 8\,H_2\,O.$$
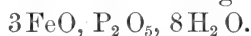
Vivianite, on heating, turns white, exfoliates, and does not decrepitate in the slightest, and, moreover, when in the crystalline form, is oxidized only to a very slight extent. It is a striking experiment to heat small crystals of Ludlamite in the ordinary atmosphere, and another portion in carbonic acid: the decrepitation is the same; but in the former case the crystals, as before observed, change their colour to a splendid bluish black, while in the latter atmosphere they become brilliantly pearly white.

This new mineral I have named *Ludlamite,* after my friend Mr. Ludlam, a gentleman who for many years has cultivated the science of mineralogy, and possesses, perhaps, the finest private collection of minerals in Great Britain. I have to return my best thanks to Mr. Talling for having first called my attention to the mineral, and for affording me specimens necessary for its examination.

I am also extremely indebted to Professor Maskelyne, F.R.S., of the British Museum, who kindly undertook the crystallographic part of the investigation, the results of which are subjoined.

The crystals of Ludlamite belong to the oblique system, the elements of the crystal being

$$1\,0\,1\,.\,1\,0\,0 = 42°\ 24\tfrac{1}{2}',\quad 1\,1\,1\,.\,0\,1\,0 = 36°\ 21',$$
$$1\,0\,1\,.\,0\,0\,1 = 37°\ 2\tfrac{1}{2}'.$$

Otherwise, the angle of the axes Z X is 79° 27′, and the para-

metral ratios $a:b:c=1:0\cdot4389:0\cdot8932$. The forms met with on the different crystals examined are

$$a, 1\,0\,0; \quad c, 0\,0\,1; \quad m, 1\,1\,0; \quad q, \bar{1}\,1\,1; \quad d, \bar{1}\,0\,1;$$
$$k, \bar{2}\,0\,1; \quad t, 2\,0\,1; \quad l, 0\,1\,1; \quad r, 1\,1\,2; \quad p, 1\,1\,1.$$

The face c is a prominent face with vitreous lustre, and is parallel to the only distinct cleavage. It presents fine striations parallel to its intersections with the faces q. The face a is small, usually triangular, and remarkable for its lustre: the faces of the form q are the most developed after the faces c, often equally developed with the latter, and then imparting to the crystal a rhombohedral aspect. They are furrowed with striation-planes parallel to the edges they form with the cleavage-plane.

The faces of the form m are brilliant but very narrow, replacing the acute edges in which a face c meets a pair of the faces q. They are somewhat rare. The faces of the form d truncating those edges of the faces q which lie in the plane of symmetry are very small, but occur not unfrequently; the composite character of the crystal, however, renders a good measurement of the angle $a\,d$ very difficult. The faces l, t, and p have been only once observed.

Owing to the difficulty of taking accurate measurements between the furrowed faces of the form q and the faces of the forms c or a, a good measurement of the angle $\bar{1}\,1\,1, 1\,1\,\bar{1}$, involving also a fair determination of the angle $\bar{1}\,1\,1, \bar{1}\,0\,1$ on one of the crystals, was employed for the third datum in determining the elements of the crystal, as given above—the other two elements being the angles $1\,0\,0, 0\,0\,1$ and $1\,0\,0, 1\,1\,0$, as given by two other crystals. Angles obtained from the measurement of four crystals were as follows:—

Calculated angles.			Measured angles.
$a\,c$	$=$	$79\mathring{9}\ 27$	$79^\circ\ 26\tfrac{1}{2}'\ \|\ 79^\circ\ 27'\ \|80^\circ\ 20'$
$c\,a'$	$=$	$100\ 33$	
$c\,d$	$=$	$46\ 23$	$45^\circ\ 56'$
$d\,a'$	$=$	$54\ \ 9\tfrac{1}{2}$	$54^\circ\ 33'\ \ \|\ 54^\circ\ 9'\|54^\circ\ 3'$
$a\,d$	$=$	$125\ 50\tfrac{1}{2}$	$125^\circ\ 27'$
$a\,t$	$=$	$26\ 31\tfrac{1}{2}$	
$t\,c$	$=$	$52\ 55\tfrac{1}{2}$	
$c\,k$	$=$	$69\ \ 2\tfrac{1}{2}$	
$k\,a'$	$=$	$31\ 30\tfrac{1}{2}$	$31^\circ\ 37'$
$a\,m$	$=$	$65\ 55\tfrac{1}{2}$	$65^\circ\ 56'\ \ \|\ 65^\circ\ 55\tfrac{1}{2}'$
$m\,m'$	$=$	$48\ \ 9$	$48^\circ\ \ 8'$
$m'\ \bar{m}$	$=$	$131\ 51$	$131^\circ\ 52'$

Calculated angles.   Measured angles.

$$
\begin{bmatrix}
a\,q &= 107^\circ\ 48' \\
q\,a' &=\ \ 72\ \ 12 \\
a\,p &=\ \ 63\ \ 56 \\
a\,l &=\ \ 85\ \ 16 \\
p\,l &=\ \ 21\ \ 20 \\
l\,a' &=\ \ 94\ \ 44 \\
l\,q &=\ \ 22\ \ 33
\end{bmatrix}
$$

$107^\circ\ 43'$ $\| 108^\circ$
$72^\circ\ 17' \| 72^\circ\ 35' \mid 72^\circ\ 39\frac{1}{2}'\ \| 72^\circ \| 72^\circ\ 22'$
$64^\circ\ \ \ 9'$
                               $85^\circ\ 23'$
                               $21^\circ\ 24'$

$$
\begin{bmatrix}
c\,m &=\ \ 85\ \ 43 \\
m\,c' &=\ \ 94\ \ 17 \\
c'\,q &= 111\ \ 36 \\
c\,q &=\ \ 68\ \ 54 \\
m'\,q &=\ \ 25\ \ 23 \\
c\,r &=\ \ 44\ \ 53 \\
r\,m &=\ \ 40\ \ 50 \\
c\,p &=\ \ 61\ \ 38\tfrac{1}{2} \\
p\,m &=\ \ 24\ \ \ 4\tfrac{1}{2}
\end{bmatrix}
$$

$86^\circ\ \ \ 6'$
$93^\circ\ 54'$ $94^\circ\ 28'$
$110^\circ\ 59' \| 111^\circ\ 52' \mid 111^\circ\ 4' \| 111^\circ\ 30'$
$69^\circ\ \ \ 1' \|\ 68^\circ\ 6'\ \text{to}\ 68^\circ\ 56' \|\ 68^\circ\ 30' \| 68^\circ\ 49'$
                               $25^\circ\ 29'$
$\| 44^\circ\ 12'$

                               $23^\circ\ 55'$

$$
\begin{bmatrix}
q\,q' &=\ \ 63\ \ 24 \\
d\,q &=\ \ 58\ \ 32 \\
q\,d' &= 121\ \ 28 \\
q\,\bar{q}' &= 116\ \ 36
\end{bmatrix}
$$

$63^\circ\ 24' \| 63^\circ\ 38'$
$58^\circ\ 32' \mid 58^\circ\ 10\frac{1}{2}' \| 58^\circ\ 14' \| 58^\circ\ 33'$
$116^\circ\ 36'$ $116^\circ\ 32' \| 116^\circ\ 13'$

$$
\begin{bmatrix}
k\,q &=\ \ 61\ \ \ 8 \\
k\,m &= 110\ \ 23 \\
q\,m &=\ \ 49\ \ 15 \\
m\,k &=\ \ 69\ \ 37
\end{bmatrix}
$$

$60^\circ\ 40'$
$110^\circ\ 19'$
$49^\circ\ 39 \| 49^\circ\ 57'$
$70^\circ\ 12'\ \ 69^\circ\ 48'$

$$
\begin{bmatrix}
p\,n &=\ \ 53\ \ 39 \\
p\,b &=\ \ 36\ \ 21
\end{bmatrix}
$$

$$
\begin{bmatrix}
c\,l &=\ \ 63\ \ 12\tfrac{1}{2} \\
l\,b &=\ \ 26\ \ 47\tfrac{1}{2}
\end{bmatrix}
$$

The crystals are generally composite—the faces of the forms $\{\bar{1}\,1\,1\}$ and $\{0\,0\,1\}$ being not only furrowed but often also tessellated, and, from this cause also, not planes; and occasionally, where the crystal is thin in the direction of the normal to c, the faces q in the zone $[c\,m]$ assume a rounded character, and the angle $q\,c$ offers various values, the limits of which are about 64° and $74^\circ\ 17'$.

Besides the very perfect cleavage parallel to the face $0\cdot0\,1$,

there is a second and more difficult cleavage parallel to the faces of the form 1 0 0.

The figures 1 and 2 represent the distribution of forms on two crystals.

Fig. 1.
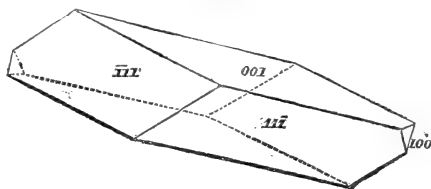
Fig. 2.
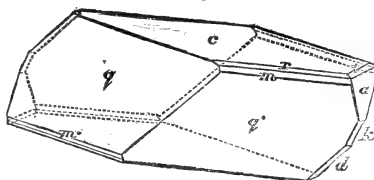
Fig. 3 is a stereographic projection of the poles of the crystal.

Fig. 3.
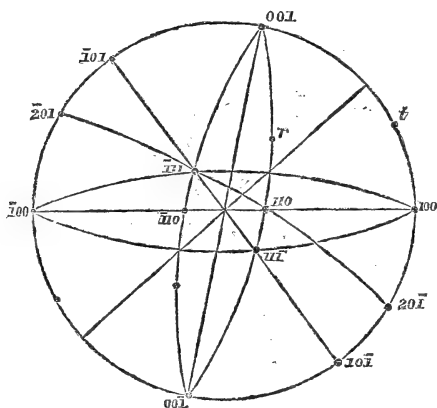
The optical character of the crystal is negative; the acute bisectors of the optic axes for different colours lie in the plane of symmetry, and are very slightly inclined on the normal to the face 1 0 0, the optic axes also lying in the plane of symmetry.

IX. *Notices respecting New Books.*

Traité d'Électricité Statique. Par M. E. MASCART, *Professeur de Physique au Collége de France.* 2 vols. Paris: G. Masson. 1876.

SINCE the publication of Riess's *Reibungs-Electricität* in 1853, enormous strides have been made in electrical science, and not least in those regions of it comprehended by M. Mascart under the title *Électricité Statique.* The theory of the potential has been widely extended and applied; and the various new forms of electrometer have given us the means of performing electrical measurements with a precision unattainable twenty-five years ago. Important researches involving new methods have been carried out and published both in this country and on the continent, while the mathematical theories connecting the observed phenomena have been developed in an admirable manner by Professor Clerk Maxwell and others.

The work before us is undoubtedly the most complete and important treatise on statical electricity, considered from the experimental point of view, that has been published since Riess's work appeared. It supplies a want widely felt. It brings before us concisely, but in a well connected and interesting manner, the results obtained by the labours of the less-recent workers, Coulomb, Poisson, Snow Harris, Faraday, &c., and, though not professing to furnish a complete history of the science, or to cite all the physicists who have contributed to its progress, describes the most recent advances made in all lines of research connected with its subject. We have here, in fact, together with the most valuable part of Riess's work, to which the author acknowledges his great obligations, a lucid and sufficiently detailed account of the ideas and methods developed since his time. Nor is Prof. Mascart satisfied with mere description or abstraction. His expositions of the researches of others are generally accompanied by discussion and criticism, always trenchant and suggestive, and for the most part, as we think, just.

The title of the book *Électricité Statique* must not be understood in its restricted sense. It includes here not only the matter comprised under this heading in the ordinary text-books, *i. e.* the general conditions of equilibrium of electrified bodies, &c., but gives an account also of the principles of the Voltaic pile, the phenomena of the thermoelectric circuit, and other matters belonging strictly to the department of dynamical electricity.

Professor Mascart regards his subject from an experimental rather than from a mathematical point of view, nearly the whole of the mathematical investigations in the two volumes being contained in two chapters, the sixth and seventh, styled respectively "Theory of Electrical Phenomena" and "Applications of the Theory."

The first of these gives a sketch of the potential theory and of the properties of the potential function. Green's transformation

formula is then explained, and its more important applications to the various relations of electric forces and potentials stated in a series of propositions. The general theory of electrical influence, and the theory of condensers of various forms, cylindrical, spherical, and plane, complete this chapter. The next chapter discusses the distribution of electricity on two spheres in various relative position, according to the calculations of Poisson, Plana, Thomson, &c., and gives an account of Thomson's theory of electrical images. It concludes with a comparison, at some length, of the phenomena of statical electricity with those of thermal conductivity. Although Poisson, Thomson, Clerk Maxwell, &c. have been largely drawn upon in this portion of the work, the author is careful to refer to those writers for details, and as a rule discusses mathematically only those particular cases which admit of experimental verification.

We proceed to give a brief account of the other portions of the work. The first chapter is preliminary; the second, third, and fourth give a very complete and admirable account of the experimental researches of Coulomb and others on the laws of electric action, the distribution of electricity on conductors of various forms, its loss by the air and insulating supports, and the theory of the proof-plane. The fifth chapter treats of the general phenomena of electric induction, the behaviour of insulating bodies with respect to the distribution of electricity in them, condensation, &c. Chapter viii., on "Instruments of Measurement and Observation," contains a classification of most of the types of electroscopes and electrometers that have ever been used, and will be found useful for reference. Though the descriptions are as a rule satisfactory, there are some exceptions: *e.g.* the descriptions and pictures of Kohlrausch's and Dellmann's electrometers are vague and inadequate; an ordinary reader with these only for his guide will probably fail to understand the instruments. Lippmann's electro-capillary electrometer is relegated to the last chapter.

The tenth, eleventh, and twelfth chapters give an account of the modes and effects of the electric discharge, mechanical, calorific, luminous, &c., and also of induction produced by the discharge. The researches of Verdet on this last subject are well described, and also the more recently published researches of Bichat. Bichat obtained powerful effects by passing the discharges of a Holtz machine through the fine wire of an induction-coil, and collecting the induced discharges manifested in the thick wire. Under these conditions the difference in tension between the two induced discharges was enormous, and the decomposition of water was effected almost as completely as if a continuous current had been employed. This decomposition M. Bichat shows to be due to the inverse induced discharge. The thirteenth chapter is on "Electrical Machines." With respect to the Holtz machine, the author regards as unnecessary the explanation of its action given by Riess by reference to "double induction." He says, in order that double induction may be produced, the insulated body must be electrified

itself, an effect which always requires a certain time, and the rotation of the plate is too rapid for the induction exercised upon the glass to play an appreciable part. Besides, the production of electrical layers of the same size upon the two faces of the moveable plate is explained in a perfectly simple way by the action of the points of the conductors and the armatures. The part played by the apertures confirms also this explanation.

A curious phenomenon connected with the Holtz machine—involving grave inconvenience when the machine is used for the purpose of charging a large Leyden battery—must have been observed by every one who has had much need to use the machine. As soon as one of the armatures has been initially excited, the Leyden jars connected with the two electrodes begin to be charged; and after the rotation has been continued for a short time, suddenly the electricity is extinguished and resumed again in the opposite direction, so that the positive electrode becomes negative and *vice versâ*. This change is detected at once by the change of form of the glow at the points of the combs and the paper armatures; each of the jars is then discharged slowly by the corresponding comb, and becomes charged with the opposite electricity to the same potential, after which the same inversion is repeated. One of the best methods for getting rid of this inconvenience is that of Holtz himself. In this arrangement the paper armatures are formed of two bands which traverse each nearly the whole extent of a quadrant of the fixed plate. On the axis of the machine, and just in front of the movable plate, is fixed a diametral conductor terminated at each extremity by metallic combs similar to the ordinary combs. This conductor, which may be placed along any diameter, is usually inclined at an angle of about 60° to the direction of the two ordinary combs of the machine. According to Prof. Mascart, the use of such a conductor, though it may slightly weaken the power of the machine, entirely prevents both inversion and extinction, so long as the movable plate is in motion, even though the electrodes are widely separated.

The last three chapters of the work are concerned with "Sources of Electricity." First are discussed the experiments of Volta, Thomson, &c. on the electrical separation produced by the contact of dissimilar metals. The author, differing in this respect from the French writers of a few years ago, entirely adopts the views insisted on by the German electricians, and in this country by Sir William Thomson and others. That is, Volta's original explanation of his fundamental experiment is adopted, though at the same time Volta's error, in supposing that there could be a flow of electricity in a circuit composed entirely of metals all at the same temperature, is pointed out. Some of Faraday's experimental objections to Volta's theory are mentioned and speedily disposed of, the author apparently deeming it unnecessary to discuss them at any great length.

Next come the experiments of Kohlrausch, Buff, Hankel, &c. on the difference of potential produced by the contact of dissimilar

bodies, and those of Branly on the absolute measure of electromotive force. The phenomena of thermo-electric circuits, though occupying much space, are not treated in so satisfactory a manner as their importance demanded, the researches of some recent writers on the subject (*e. g.* Prof. Tait) not being even alluded to.

Clausius's hypothesis respecting the electromotive force in a thermoelectric circuit almost disposes of itself. Clausius endeavoured to apply Carnot's theorem (relative to heat-engines) to the quantities of heat absorbed or disengaged at the two junctions in a closed circuit of two metals, and showed as a result that the electromotive force must be proportional to the difference of temperature. It is found, however, experimentally that this proportionality does not hold except within very narrow limits of temperature. Prof. Mascart says :—" Carnot's theorem is only true for those heat-engines which are reversible; and thermoelectric currents do not present any character of reversibility which justifies Clausius's calculation."

A Treatise on the Trisection of an Angle of thirty degrees, and of any other plane Angle. By BERNARD TINDAL BOSANQUET. London: Effingham Wilson, Royal Exchange. 1876. (Pp. 20.)

It is well known that the problem of the trisection of an angle is not simply to divide an angle or circular arc into three equal parts, but to effect the trisection by means of the straight line and circle only. Mr. Bosanquet would probably not have published his tract if he had distinctly understood this simple fact. As it is, his work consists of two parts : in the former he proposes a method of trisecting an angle of 30° by means of the straight line and circle ; in the latter he gives a method of trisecting any angle by means of a hyperbola.

The first of these methods is as follows :—Draw a circle, and at its centre B place an angle CBD of 120° ; draw a radius BE parallel to the chord CD ; bisect BE in G and draw GH at right angles to BE to meet in H a tangent to the circle at C ; with centre H and radius HG, describe an arc of a circle to cut the circumference of the first circle in K ; join BK, GK. Mr. Bosanquet asserts that GHK − 3 GBK = 2° 30′ ; and if it be so, an angle of 10° can then be constructed. He expresses (Preface, p. iii) a well-founded doubt as to the exactness of this solution. In fact, as GHK is slightly greater than 10°, and GBK very slightly greater than a quarter of GHK, the result 2° 30′ must be nearly right ; but the only evidence produced for its exactness is that, when the angles are calculated Trigonometrically, the result comes to 2° 30′ within a second—which, of course, is not the sort of evidence required; and, besides, we believe, as a matter of fact, that it is about 1″ short of 2° 30′ ; so that our author may be held to have failed in the first part of his work, in which he observes the restrictions implied in the problem. In the second part he effects the trisection of any arc of a circle by means of a hyperbola ; but as it was already well known that the solution could be effected by Conic

Sections, his construction, though correct, is of little importance. The author speaks of himself as one whose knowledge of mathematics is " somewhat circumscribed." We do not doubt that he has found the composition of the tract a useful exercise; but the publication of it, we venture to suggest, was entirely unnecessary.

The Earth Supported by Vapour. By G. T. CARRUTHERS, *M.A.*, *Chaplain of Naypur, India.* Nagpur: printed at the Chief Commissioner's Office Press, 1876. (Pp. 23, 8vo.)

We do not doubt that were Professor de Morgan still living he would have found a place for Mr. Carruthers in his Budget of Paradoxes. That he is well qualified for admission will be plain from a brief statement of the object of the tract before us, which the author gives in the following words :—" Sir Isaac Newton discovered a fact which no one now doubts, that the solar system is governed by a law of attraction. He himself held an opinion for a long time that this attraction was not an inherent occult power in matter to attract other matter, but that there might be some physical cause of such apparent attraction. The present paper is written to suggest that the physical cause is the condensation of vapour of more or less tenuity intervening between the heavenly bodies and surrounding them " (p. 1). His theory, then, is that gravitation or the weight of bodies is caused by the condensation of the watery vapour surrounding them. When such a theory as this is propounded, it is hard to know what to say : one might ask what chance there is of watery vapour existing in the form of watery vapour in the interplanetary spaces, *e. g.* in the space between Uranus and Neptune. But perhaps the consideration of our author's reasoning applied to a particular case will throw sufficient light on the subject. Here is his account of the reason of the fall of Newton's famous apple :—" The vapour on the earth's surface is in perfect equilibrium from the various pressures to which it is subject; but when an apple, broken from its branch, is left unsupported in the air, the vapour between it and the earth cannot support this additional weight . . . there is, therefore, a condensation of the vapour, and the apple falls from the forward push of the vapour above it " (p. 5). Surely our author ought to maintain that the numerical value of the accelerative effect of gravity is dependent on the reading of the hygrometer; but why should we expect any sort of consistency in a theory, according to which the weight of the body is produced by the condensation of the vapour, and the condensation of the vapour by the weight? Our author having considered the apple, goes on to discuss the case of "a feather in a vacuum instrument." But it is plainly unnecessary to give any further account of a tract written apparently under the influence of a sort of midsummer madness.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. ii. p. 468.]

April 27, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

THE Bakerian Lecture, "On the Gaseous State of Matter," was delivered by Prof. T. Andrews, LL.D., F.R.S., Vice-President of Queen's College, Belfast.

After referring to certain modifications in his former method of working at high pressures, the author describes some preliminary experiments which were undertaken to determine the change of capacity in the capillary bore of the glass tubes under the pressures employed. From these experiments it appears that, on raising the pressure from 5 to 110 atmospheres, the capacity was increased for each atmosphere by only 0·0000036, and that this change of capacity was chiefly due to compression of the internal walls of the glass tube. Another set of experiments was made to ascertain whether air or carbonic-acid gas is absorbed at high pressures to any appreciable extent by mercury. For the method of operating and other details reference must be made to the original memoir; but the general result is that no absorption whatever takes place, even at pressures of 50 or 100 atmospheres. The pressures are given according to the indications of the air-manometer in the absence of sufficient data (which the author hopes will be soon supplied) for reducing them to true pressures. In the mean time it is probable, from the experiments of Cailletet, that the indications of the air-manometer are almost exact at 200 atmospheres, and for lower pressures do not in any case deviate more than $\frac{1}{80}$ from the true amount.

In a note which was published last year in the 'Proceedings' of the Society (No. 163), it was stated that the coefficient of expansion (*a*) for heat *under constant pressure* changes in value both with the pressure and with the temperature. The experiments on this subject are now completed, and are described at length in this paper. The final results will be found in the two following Tables. In the first Table the values of *a* are referred to a unit volume at 0° and under one atmosphere. In the first column the pressure *p* in atmospheres is in terms of the air-manometer.

p.	*a* (0°–7°·5).	*a* (0°–64°).	*a* (64°–100°).
17·09	. . . . . .	0·005136	0·004747
20·10	0·00607	0·005533	0·004958
22·26	. . . . . .	0·005811	0·005223
24·81	0·00700	0·006204	0·005435
27·69	0·00782	0·006737	0·005730
31·06	0·00895	0·007429	0·006169
34·49	0·01097	0·008450	0·006574

For higher pressures than 35 atmospheres a different unit volume must be taken, on account of the liquefaction of the carbonic acid. The next Table contains the values of a, *referred to the unit volume at* 64°, between 64° and 100° at constant pressures up to 223 atmospheres.

p.	α (64°–100°).	p.	α (64°–100°).
17·09	0·003572	46·54	0·004946
20·10	0·003657	54·33	0·005535
22·26	0·003808	64·96	0·006512
24·81	0·003892	81·11	0·008033
27·69	0·004008	106·90	0·013150
31·06	0·004187	145·50	0·018222
34·49	0·004266	223·00	0·008402
40·54	0·004596		

As the value of α changes with the temperatures, the coefficients given above are average coefficients for the ranges of temperature specified. It will be observed that the value of $\boldsymbol{\alpha}$ increases with the pressure until a very high pressure is attained, when it changes its direction and diminishes. This apparent anomaly depends upon the carbonic acid at the high pressure having passed from the gaseous state proper to the intermediate conditions, which the author has formerly described as establishing a continuity between the gaseous and liquid states of matter.

If we designate by $\boldsymbol{\alpha}'$ the coefficient of elastic force when a gas is heated under a constant volume, we shall have in the case of a perfect (ideal) gas

$$\alpha = \alpha'.$$

In the ordinary gaseous state the value of $\boldsymbol{\alpha}'$ diverges widely at high pressures from that of α, as will appear from the two following Tables. In the first Table the values of α' are referred to a unit pressure at 0°, and p is the initial pressure in atmospheres.

p.	α' (0°–6°·5).	α' (0°–64°).	α' (64°–100°).
16·42	. . . . . .	0·004754	0·004607
21·48	0·00537	0·005237	0·004966
25·87	0·00588	0·005728	0·005406
30·37	. . . . . .	0·006357	0·005861
33·53	0·00734	0·006973	0·006334

In the next Table the value of α' between 64° and 100° referred to a unit pressure at 64° is given for a large range of pressure. Both the initial and final pressures are given.

p.	α' (64°–100°).	p.	α (64°–100°).
21·42 24·19	0·003526	48·40 56·16	0·004387
28·65 32·60	0·003718	67·65 80·99	0·005392
35·29 40·44	0·003956	94·27 118·60	0·007018
42·74 49·25	0·004166		

As the general result of this investigation, it follows that in the ordinary gaseous state the law of Gay-Lussac does not hold good, either in the case of α or of α', and that the dilatation *by heat of a body in the ordinary gaseous state, whether measured by its expansion under constant pressure or by the increase of elastic force under constant volume, is not a simple function of the initial volume or initial elastic force, but a complex function changing with the temperature.*

In the second part of the paper the general properties of the ordinary gaseous state are considered. As the basis of this inquiry, the following Table of the compressibility of carbonic acid at 6°·5, 64°, and 100° is given, in which ϵ is the ratio of the observed volume of the carbonic acid at the pressure p and temperature t' to its volume under one atmosphere at the same temperature t'.

p.	$t'=6°\cdot5$, ϵ.	p.	$t'=64°$, ϵ.	p.	$t'=100°$, ϵ.
12·01	$\frac{1}{12\cdot95}$	17·60	$\frac{1}{18\cdot57}$	20·17	$\frac{1}{20\cdot98}$
13·22	$\frac{1}{14\cdot37}$	20·36	$\frac{1}{21\cdot65}$	22·37	$\frac{1}{23\cdot35}$
14·68	$\frac{1}{16\cdot13}$	22·56	$\frac{1}{24\cdot13}$	24·85	$\frac{1}{26\cdot09}$
17·09	$\frac{1}{19\cdot12}$	25·06	$\frac{1}{27\cdot08}$	27·76	$\frac{1}{29\cdot32}$
20·10	$\frac{1}{23\cdot03}$	28·07	$\frac{1}{30\cdot64}$	31·06	$\frac{1}{33\cdot05}$
22·26	$\frac{1}{25\cdot96}$	31·39	$\frac{1}{34\cdot67}$	34·57	$\frac{1}{37\cdot09}$
24·81	$\frac{1}{29\cdot52}$	34·92	$\frac{1}{39\cdot08}$	40·09	$\frac{1}{43\cdot54}$
27·69	$\frac{1}{34\cdot03}$	40·54	$\frac{1}{46\cdot34}$	45·99	$\frac{1}{50\cdot63}$
31·06	$\frac{1}{39\cdot59}$	46·56	$\frac{1}{54\cdot57}$	53·81	$\frac{1}{60\cdot30}$
34·49	$\frac{1}{45\cdot80}$	54·33	$\frac{1}{65\cdot97}$	64·27	$\frac{1}{73\cdot97}$
		64·96	$\frac{1}{83\cdot44}$	80·25	$\frac{1}{96\cdot65}$
		81·11	$\frac{1}{114\cdot0}$	105·69	$\frac{1}{37\cdot6}$
		106·88	$\frac{1}{185\cdot5}$	145·44	$\frac{1}{218\cdot0}$
		145·54	$\frac{1}{325\cdot9}$	223·57	$\frac{1}{379\cdot3}$
		222·92	$\frac{1}{446\cdot4}$		

If we put $\rho = \epsilon p$, and calculate the values of ρ for each of the above experiments, it will be found that these values diminish regularly as the pressure increases at each temperature, with the exception of the last observation at 64°, where the value of ρ is greater than at the preceding observation. This change of direction in the value of ρ is explained by the circumstance already mentioned, that the carbonic acid at the higher pressure has been reduced to the liquid volume (although no liquefaction has taken place) in passing through the conditions of matter intermediate between the gaseous and liquid states.

The relations between the volumetric curves in the ordinary gaseous state for different temperatures are determined by means of what the author calls the homologues or homologous points of those curves—that is, the points in any two isothermals where the values of ρ are equal, or

$$p\,v = p'\,v'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(A)}$$

From a careful analysis of the experiments described in this communication and in his former Bakerian Lecture, the author shows that the ordinary gaseous state is characterized by the ratio of the external pressures for all the homologues of any two given temperatures being constant, or

$$\frac{p}{p'} = \mathfrak{m}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B)}$$

where $\mathfrak{m}$ is a constant. It must be carefully observed that v and v' in equation (A) are the volumes of the gas on different isothermals.

It follows from these results that, in the case of a body in the ordinary gaseous state, if the relations of pressure and volume are known at any one temperature, the corresponding relations at any other temperature can be calculated from the observation of a single homologue at the second temperature. Thus the whole relations of volume and pressure will be known from a set of primary observations at a fixed temperature and the determination of one homologue for each of the other temperatures.

The general form of the primary curve itself, or curve exhibiting the relations of pressure and volume at a given temperature, is next investigated; and the author finds, as the result of all his experiments under very varied conditions of temperature and pressure, that the value of $\epsilon(1-\rho)$ is constant for the same temperature. Hence we have for the third equation of the ordinary gaseous state

$$v(1 - pv) = c. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C)}$$

From this last equation the relations of homologous points, as defined by equations (A) and (B), may be deduced.

The author concludes with some general observations on the action of the internal attractive forces in the ordinary gaseous state, and also on the resistance to diminution of volume, which, under certain conditions, modifies largely the effects both of external pressure and of internal attraction.

May 11.—Dr. Günther, M.A., Vice-President, in the Chair.

The following communication was read:—

" Condensation of Vapour of Mercury on Selenium in the Sprengel Vacuum." By R. J. Moss, F.C.S., Chemical Laboratory, Royal Dublin Society.

In the course of experiments on the electrical conductivity of selenium, a cylindrical bar of this substance in the vitreous state was enclosed in a glass tube which was attached to the exhaust-tube of a Sprengel pump. The bar of selenium was 45 millims. long and 3·5 millims. in diameter. Platinum wires were attached to the ends of it and passed through the sides of the glass tube. The tube was exhausted, and allowed to remain attached to the pump for four days. It was now found that the selenium had acquired a conductivity greatly exceeding that of the pure element in its most highly conducting condition. The experiment was repeated with the intention of observing the time required to produce conductivity. In forty-two hours the needle of a highly sensitive galvanometer was slightly deflected when the selenium was placed in the circuit of ten Leclanché cells. The conductivity of the selenium increased rapidly for four days, when the experiment was unavoidably interrupted. On admitting air to the tube no change of conductivity was observed. The selenium was unaltered in appearance, even when examined microscopically. On breaking the bar it was found that the conducting-film was entirely superficial ; it was not removed by rubbing forcibly with a cloth. Dilute nitric acid also failed to remove it. Bibulous paper moistened with solution of silver ammonio-nitrate was not stained by it (Merget, ' Comptes Rendus,' vol. lxxiii. p. 1356). It therefore appears highly probable that the film does not consist of uncombined mercury. As it has not hitherto been known that mercury combines with selenium at ordinary temperatures, a bar of selenium was immersed in mercury and allowed to remain undisturbed for six months. At the end of this time it was found that the selenium was coated with a highly conducting film. I could not detect any difference between this film and those produced in the Sprengel vacuum. An attempt was now made to estimate the quantity of mercury required to produce the observed conductivity. A bar of selenium 125 millims. long and 2 millims. in diameter, having platinum wires fused into each end, was enclosed in a glass tube, containing also a minute globule of mercury about 0·5 millim. in diameter. The tube was exhausted by means of the Sprengel pump, and then hermetically sealed and detached from the pump. In 92 hours the bar began to conduct, and the conductivity increased rapidly from day to day for four days. On the fifth day, no increase being observed, it was supposed that air had leaked into the tube ; and on examining it a flaw, which would account for the leakage, was detected. The tube was therefore again attached to the pump, exhausted, and again sealed, the defective portion being

removed. The conductivity of the bar again increased from day to day, and is still steadily but slowly increasing (eleven days after the second sealing of the tube). Although the bar of selenium now possesses a comparatively low resistance, I cannot detect the slightest alteration in the size of the minute globule of mercury which has supplied the material for the conducting-film, extending over a surface one thousand times greater than that of the globule.

The granular modification produced by subjecting vitreous selenium to a temperature of 100° C. for three hours also acquires a great increase of conductivity when exposed to the vapour of mercury in the Sprengel vacuum.

As it is possible at any moment to arrest the formation of these conducting-films, bars of selenium of any given high resistance may be obtained in this way with great certainty and accuracy.

May 18.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

The following communication was read:—
"Absorption-Spectra of Iodine."   By Sir John Conroy, Bart., M.A.

Iodine, as is well known, when in very thin layers, appears red by transmitted light; and when in solution the colour of the liquid depends not only on the amount of iodine contained in it, but also on the nature of the liquid in which it is dissolved.

Schultz-Sellack has pointed out (Pogg. Ann. vol. cxl. p. 334) that the liquids in which iodine is soluble may be divided into two classes :—first, those with which it gives reddish-brown solutions, like alcohol; and secondly, those with which it gives violet ones, as bisulphide of carbon; and also that the colours of these two solutions correspond respectively with the colour of solid iodine, when seen by transmitted light, and with that of iodine vapour.

Andrews (Brit. Assoc. Report, 1871) has also remarked that iodine vapour and the solution of iodine in bisulphide of carbon are dichroic, while such is not the case with its solution in alcohol.

As I am not aware of any other observations on the absorption of light by iodine in solution or in the solid state having been published, I have the honour of having an account of some experiments I have recently made on this subject communicated to the Royal Society.

For these observations I have used one of Browning's spectroscopes with a single dense-glass prism of 60°, as with a greater amount of dispersive power it became more difficult to observe the beginning and end of the absorption. The spectroscope was firmly screwed to the wall of the room, with the collimator pointing vertically downwards, the light from a paraffin-lamp being reflected along it by a mirror—the width of the slit and the position of the mirror and lamp remaining unaltered during the course of the experiments, in order that the different absorption-spectra should, as far as possible, be comparable with each other. The solution whose absorption was to be observed was contained in a small

beaker, supported by the ring of a retort-stand between the mirror and the slit of the collimator.

This arrangement was adopted in order to be able to observe the absorption through various thicknesses of the same solution, without having to use a wedge-cell, as some of the liquids in which iodine is soluble act very quickly on the cement with which such cells are fastened together.

A vertical scale was attached to the beaker, so that by gradually pouring a solution into it, the absorption through different known thicknesses could be observed, the solutions of iodine in bisulphide and tetrachloride of carbon being covered with a thin layer of water to prevent their evaporating.

When the absorption-spectra of solid and liquid iodine were to be observed, the beaker was replaced by a large cork which fitted the ring of the retort-stand, and through which a hole had been bored in a line with the axis of the collimator, and the glass slips between which the iodine had been melted laid on this. In the case of the liquid iodine, the low conductive power for heat of the cork retarded the cooling of the glass, and facilitated the observation of the absorption.

The telescope of the spectroscope, the eyepiece of which was furnished with cross-wires, was carried by an arm moving over a divided arc; and the position of 10 of the principal lines in the solar spectrum having been observed, from these measurements, and from the wave-lengths of the same lines, as determined by Ångström, a curve was constructed, by means of which the readings of the spectroscope were reduced to wave-lengths.

Solid Iodine.

Layers of iodine sufficiently thin to be transparent can be readily obtained, as Schultz-Sellack has remarked, by squeezing melted iodine between two pieces of flat, well-polished glass : it is only necessary to place a small fragment of iodine between two pieces of glass which have been previously well cleaned with alcohol, and heat them over a spirit-lamp till the iodine melts, and then press them together. I have obtained the best results by heating the iodine till it just melts, placing the pieces of glass on a smooth block of wood and squeezing them together with a flat cork.

The layers of iodine thus obtained are not usually of uniform thickness ; and, in addition to this, they contain so little iodine that I was unable to determine their thickness by ascertaining the weight and area of the film. When seen, however, by reflected and transmitted light, the iodine film usually appears surrounded by coloured rings ; and as these alter their position and shape when the glass slips are pressed together, they must be due to a thin layer of air, and not to any substance adhering to the glass ; and consequently the layers of iodine are probably less than ·00004 inch, or ·001016 mm., in thickness.

When seen by transmitted light, these layers of iodine vary in

colour from a deep brownish red, through different shades of brown, to a more or less pure yellow, according to the thickness and nature of the film; for, as is shown in the paper " On the Polarization of Light by Crystals of Iodine " (Proc. Roy. Soc. vol. xxv. p. 51), the colour of the transmitted light apparently does not depend solely on the thickness of the layer of iodine through which it passes. These films correspond in colour with alcoholic solutions of iodine of different strength; and the absorption-spectra are very similar—the whole of the blue end of the spectrum being cut off, and the absorption extending further and further towards the less-refrangible end of the spectrum, as the thickness of the film increases, till at length only light having a wave-length of about 650 (in " tenth-metres "), or slightly more refrangible than the C line, passes through; and a very slight increase in the thickness of the film is sufficient to stop this also.

Fig. 4 shows the appearance with a film of medium thickness, the strong absorption ending at a point about one third of the distance between E and D, together with a certain amount of darkening extending to about D, and represents the mean results from measurements of the absorption-spectra of seven different films of iodine.

Liquid Iodine.

With a little careful management it is usually possible to melt one of these thin layers of iodine by heating it over the flame of a spirit-lamp, without causing it either to be dissipated in vapour or to run together. The liquid iodine appears to be more transparent than the solid; for a layer which appears a deep red colour by transmitted light when hot, sometimes becomes perfectly opaque on cooling. When hot it is also of a deeper red than when cold; and the spectroscope shows that while the less-refrangible rays of the spectrum are freely transmitted, there is more absorption of those of mean refrangibility than is the case with solid iodine.
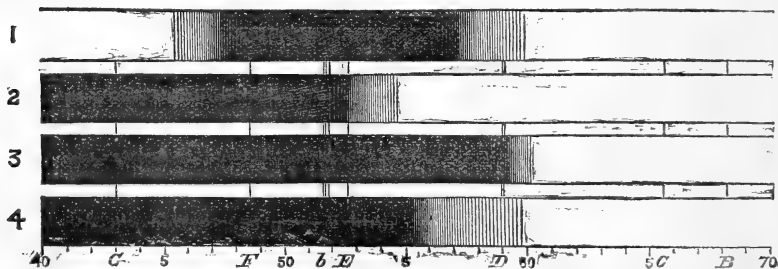
Fig. 3 shows the absorption-spectrum as deduced from five observations. Just as is the case with solid iodine, light of a wavelength of about 650 suffers the least amount of absorption during its passage through a layer of iodine.

Iodine in Solution.

As I have before mentioned, the liquids in which iodine is soluble may be divided into two classes:—first, those with which it forms brownish-red solutions, as alcohol, ether, ethyl bromide, Dutch liquid, benzole, glycerine, potassium iodide in an aqueous solution, hydrogen chloride, &c.; second, those with which it forms violet solutions: this latter class is less numerous, and as far as I have hitherto been able to ascertain by actual experiment, only consists of the following substances—the bisulphide, tetrachloride, and monochloride of carbon, chloroform, phosphorous terchloride, tin tetrachloride, and under certain circumstances, as will be hereafter mentioned, hydrogen sulphate. In Watts's ' Dictionary of Chemistry,' vol. i. p. 881, it is stated that the solution of iodine in chloral has a purple colour.

In order to obtain solutions of iodine of a known strength, some of the liquids in which it is soluble being very volatile, a small glass weighing-tube closed with a cork was nearly filled with iodine which had been crushed small with the edge of a platinum spatula. A stoppered flask holding 25 c. c. was filled with the liquid in which the iodine was to be dissolved; and the tube having been weighed, a small quantity of iodine was shaken out of it, and the tube again weighed, and so on, till the desired amount had been added to the liquid.

In this way solutions of iodine in bisulphide and tetrachloride of carbon, chloroform, alcohol, and in aqueous solutions of potassium iodide were prepared, and the absorption produced by different thicknesses of these solutions observed.

Solutions of iodine in alcohol, and of iodine in an aqueous solution of potassium iodide, of different strengths were then prepared, and the absorption produced by layers of these solutions, varying in thickness from 5 mm. to 25 mm., was observed.

Solutions of iodine in both liquids were, when of equal strength, of the same colour; and the absorption-spectra were similar, and also similar to that of the solid iodine, except that the absorption did not appear to extend quite so far down the spectrum; but as the transition from strong absorption to none at all is a very gradual one, it is somewhat difficult to determine the exact point at which the absorption ends. Fig. 2 represents the mean results obtained from thirty observations through different thicknesses of the alcohol solutions.

Iodine dissolved in bisulphide of carbon absorbs most strongly light of mean refrangibility; and the absorption-spectrum resembles that of the vapour of iodine as represented in the map to M. Thalen's memoir, ' Le spectre d'absorption de la vapeur d'Iode' (Upsal, 1869), except that the absorption is continuous, and does not extend quite so far towards the less-refrangible end of the spectrum.

Fig. 1 represents the mean result of eighty-one measurements of the position of the absorption produced by different thicknesses of

these solutions. The rays whose wave-lengths lie between 450 and 560 (approximately) are first absorbed ; and as the thickness of the layer or the concentration of the solution is increased, the absorption extends towards both ends of the spectrum, though more rapidly towards the less-refrangible one.

Just as was the case with the solution of iodine in alcohol, and with solid and liquid iodine, light of a wave-length of about 650 passes unabsorbed through a considerable thickness of the solution ; but a thickness which is sufficient to stop the whole of the red rays still allows the blue and violet ones to pass ; hence, whilst dilute solutions of iodine in bisulphide of carbon and other liquids of that class appear of a kind of red, when the light passes through a greater thickness, or the solution is more concentrated, they appear blue or violet.

Iodine is insoluble in cold hydrogen sulphate ; but when some fragments of iodine are placed in a test-tube, partially filled with strong hydrogen sulphate, and the tube heated, the iodine first melts, and then gradually colours the liquid, till it becomes about the same tint as a very dilute solution of iodine in bisulphide of carbon.

·0100 grm. of iodine was placed in a test-tube containing 25 c. c. of strong hydrogen sulphate, and the test-tube carefully heated over a spirit-lamp until the whole of the liquid iodine had disappeared ; the acid appeared of the same colour as a solution of iodine in chloroform, containing about the same amount of iodine. The colour of the acid did not alter on cooling ; after standing 24 hours the upper layer of acid for a depth of about one centimetre from the surface had become colourless ; but at the end of five months the acid in the lower part of the tube was still pink, the upper half having become colourless, and a small quantity of a black powder having settled at the bottom.

Only a very small quantity of iodine can be held in solution by the hydrogen sulphate when cold, as any excess separates out in minute crystals.

It does not appear probable that the difference in the colour of the solutions which iodine forms with liquids of these two classes depends on any chemical fact, as both classes contain substances of very dissimilar chemical composition. I have not, however, as yet been able to ascertain any common property possessed by all the liquids of either class, beyond (as, indeed, is obvious) that all those in which iodine forms violet solutions are volatile liquids of high specific gravity.

It has been shown by various observers (H. Morton, Pogg. Ann. vol. clv. p. 573 ; Hagenbach, Pogg. Ann. vol. cxlvi. p. 533 ; Kraus, ' Chlorophyllfarbstoffe,' p. 53) that the position of the absorption-bands of substances in solution vary to a certain extent with the liquid in which they are dissolved ; but this would appear to depend on some other cause ; for, in addition to the displacement being small, it differs in amount with different liquids ; whilst in the case of iodine, as far as I have been able to observe, the posi-

tion of the absorption is the same for all the liquids belonging to one of the two classes. The action on light of iodine dissolved in alcohol greatly resembles the effect it produces when in the solid state; whilst the absorption of its solution in carbon bisulphide, and in other liquids of that class, bears, as has been pointed out to me by Professor Stokes, the same relation to the absorption-spectrum of the vapour as the spectrum of the solution of a coloured gas (nitrogen peroxide for example) does to that of the gas.

GEOLOGICAL SOCIETY.

November 22, 1876.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read :—

1. " On the Pre-Cambrian (or Dimetian) Rocks of St. David's." By Henry Hicks, Esq., F.G.S.

Referring to the ridge of pre-Cambrian rocks, which he described in a former paper as running down the St.-David's promontory, and as previously supposed to consist of intrusive syenite and felstone, the author stated that he had now found it to be composed exclusively of altered sedimentary rocks of earlier date than the Cambrian deposits, the conglomerates at the base of which are chiefly made up of pebbles derived from these rocks. Recent investigations had led him to the conclusion that the main ridge was composed of two distinct and decidedly unconformable formations, the older of which, composed of quartzites and altered shales and limestones constituting the centre of the ridge, has a N.W. and S.E. strike, and dips at a very high angle; whilst the newer series, consisting of altered shales, and having at its base a conglomerate composed of pebbles of the older rock, has a strike nearly at right angles to that of the latter. For the former he proposed the name of Dimetian, and for the latter that of Pebidian. The author indicated the points of resemblance between these pre-Cambrian Rocks and the Laurentian of Canada, the Malvern Rocks, and others in Scotland and elsewhere, but thought it safer at present to abstain from attempting any definite correlation of them. The exposure of the older, or Dimetian, series led the author to ascribe to those rocks a thickness of at least 15000 feet : the upper, or Pebidian rocks, which flank both sides of the old ridge through a great portion of its length, are apparently of considerably less thickness; but they are in most parts more or less concealed by Cambrian deposits overlying them unconformably. Running nearly parallel with Ramsey Sound is another large mass of the author's Pebidian rocks; and at the south-western extremity of Ramsey Island they compose a bold hill almost 400 feet high; and on the east side of this a fault, with a downthrow of at least 14,000 feet, has brought the Arenig beds into contact with the pre-Cambrian rocks.

2. " On the Fossil Vertebrates of Spain." By Prof. Salvador Calderon.

This paper contained a few introductory observations on the

study of the fossil vertebrates of Spain, followed by a classified list of the species (68 in number) which have been recognized in that country. The author particularly remarked on the occurrence of *Sivatherium* and *Hyœnarctos* in Spain, on the finding of remains of the Mammoth in that country, and on the presence in the peninsula at a late period of *Bos primigenius*. The earliest known Spanish vertebrates have been obtained from the Carboniferous formation.

December 6, 1876.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

The following communication was read :—
" On the Intrusive Character of the Whin Sill of Northumberland." By W. Topley, F.G.S., Assoc.Inst.C.E., Geological Survey of England and Wales, and G. A. Lebour, F.G.S., Lecturer on Geological Surveying at the University of Durham College of Science, Newcastle-on-Tyne.

The Carboniferous Limestone series of the north of England contains a bed (or beds) of basalt, known as the " Whin Sill," regarding the nature of which opinion has long been divided. Some writers regard it as truly interbedded and contemporaneous ; others look upon it as intrusive, and as having been forced laterally between the planes of bedding. The latter opinion is that held by the authors, who stated that through South and Mid Northumberland there can be no doubt as to the intrusive character of the Whin Sill. This conclusion can be established by the line of outcrop of the trap, and also by the evidence of individual sections.

A review of the literature on the subject was given by the authors, showing that the opinions of geologists are very much divided as to the nature of the Whin Sill. But amongst the practical miners of the north of England there are very few who will admit any doubt that the Whin lies evenly, and at one constant horizon, amongst the strata. Clear cases to the contrary are looked upon as merely local variations, possibly due to successive eruptions of submarine lava. The Whin Sill serves them as a definite line ; and the limestone next above it is always called the " Tyne Bottom Limestone." The question is thus of considerable economic importance. It is also of interest in reference to the volcanic history of Britain and to classification.

Prof. Phillips took the Whin Sill as the base of the Yoredale Series ; the Great Limestone he regarded as its top. But the work of the Geological Survey has shown that the Whin Sill lies at different horizons in different places ; sometimes it even lies above the Great Limestone itself. In other words, *the Whin Sill, which is supposed to mark the base of the Yoredale Series, sometimes lies above the limestone which forms the top of that series.*

With the disappearance of the supposed base-line of the Yoredales goes also any good reason for drawing a line here at all. The so-called " Tyne Bottom Limestone " cannot be traced definitely through Northumberland ; and the beds above and below this horizon have the same general character.

The authors traced the Whin Sill through Northumberland, as far

north as Dunstanborough Castle, showing the varying positions at which it occurs in the Limestone series, and noting points of interest in some of the sections. The Whin shifts its position amongst the strata to the extent of 1000 feet or more. It frequently comes up in bosses through the bedded rocks, and bakes the beds above it quite as much as those below, especially when those beds consist of shale.

As to the age of the Whin Sill, nothing definite can be said. It is frequently thrown by faults and lodes. There is no certain case of its being unaffected by faults which throw the neighbouring rocks, although there are a few doubtful cases which seem to point in this direction. As the Whin Sill does not approach the Permian area of Durham, the fact that some of the faults there are believed to be pre-Permian cannot be applied as a test of age in this case.

In other districts in Britain in which intrusive basaltic sheets occur amongst the Carboniferous rocks, there is good reason to believe that in most cases they are pre-Permian, or at least pre-Triassic. Whether or not this be the case with the Whin Sill cannot be determined. No light is thrown on this question by the composition of the rock. Mr. Allport has shown that it resembles, in all essential characters, the basalts of other Carboniferous districts, some of which are possibly contemporaneous, some certainly intrusive.

XI. *Intelligence and Miscellaneous Articles.*

ON THE PORTATIVE FORCE OF HORSE-SHOE MAGNETS.
BY V.-S.-M. VAN DER WILLIGEN.

TO saturate horse-shoe magnets, I place them vertically with their poles upon the poles of a Ruhmkorff coil, the circuit of which I open and close three or four times successively; the magnetism of my magnets, of the usual dimensions, has then attained its maximum, even of supersaturation. After the last opening I slide the magnet, carefully and without lifting it, toward the edges of the polar planes of the electromagnet. Arrived at the margin I place the armature, well cleaned, before the magnet, slowly inclining the latter, while its poles still remain in contact with those of the electromagnet. As soon as the armature has closed the magnet, this can be raised without the slightest effort; its carrying force is then nearly one third greater than the usual permanent portative force of the best magnets of M. van Wetteren*.

* The manufacturers of magnets, at the time when touching with permanent magnets was the only method known, had already noticed this state of supersaturation of horse-shoe magnets, which is dissipated at the first pulling off of the armature. Haecker even says that the portative force in this state is double the permanent force—which agrees with my observations, since the permanent portative force of these magnets is only two thirds of that of the magnets of M. van Wetteren.

It is precisely this state of supersaturation which I have regarded (I believe, the first) as the general case, governed by a very simple law; it constitutes the starting-point of my memoir, for the ulterior researches upon the state of permanence. I think it will define the researches of the makers of permanent magnets, by showing them the maximum which can be attained by them. I have sought the empiric formula which connects the portative force of supersaturation with the dimensions of the magnet well forged and tempered, whatever may be its origin. I consider the state of permanence as the special case, variable from one magnet to another, with the origin and with the conditions of making of the steel. Observing that the maxima of M. Jamin's intensity-curve, which for the state of supersaturation are in the polar planes, spontaneously recede from those planes for the state of permanence, I suspected that there was a connexion between this displacement and the passing from the one state to the other. I measured, for the first state, the distance of these maxima upon the median line—that is to say, the total length l of the horse-shoe taken as a straight bar. Afterwards I measured the distance L of these maxima for the second state upon the median line; and I have named it the *reduced length of the magnet.* I then calculated the permanent portative force p by aid of the portative force P of supersaturation, multiplying this latter by $\left(\dfrac{L}{l}\right)^{\frac{4}{3}}$; I thus obtained a new empiric formula for p.

Calling R the weight of the magnet, and C a constant, Daniel Bernoulli gave the formula $p = CR^{\frac{2}{3}}$, which he found accurate as long as the magnets compared were of like form. Verified afterwards by Geuns[*], Haecker, Elias, and others, this formula has further been found approximately applicable to magnets of different forms; p and R being given in kilogrammes, the highest value of the constant C, attained only by the magnets of M. van Wetteren, is from 21 to 22. My empiric formulæ adapted to differences of form are

$$P = AO\sqrt{s}\sqrt[4]{\frac{l}{\sqrt{s}}}, \dots\dots\dots\dots\text{(I.)}$$

and

$$p = BO\sqrt{s}\sqrt[4]{\frac{L}{\sqrt{s}}}\cdot\frac{L}{l}, \dots\dots\dots\dots\text{(II.)}$$

in which A and B are coefficients to be determined by experiment, O and s are the circumference and the surface of the polar faces, l and L are the length and the reduced length of the magnet.

In my memoir will be found the observations which served me in the research of these formulæ:—(1) the results found for nineteen simple magnets made by M. van Wetteren with a new steel which he obtains from commerce, the weights of which vary from 0·334

[*] *Mémoire sur les Aimants,* &c., Venlo, 1862.

to 2·169 kilogrammes; (2) the results found for ten simple mag-
nets made of other sorts of steel by M. van Wetteren, or made of
unknown kinds by Haecker, Wenkebach, and Van Malderen, all
therefore more or less different from the first, and of weights vary-
ing from 0·492 to 3·033 kilogrammes. Formulæ I. and II. being
judged applicable to compound magnets, I have given (3) the results
which I found for ten magnets of M. van Wetteren, each composed
of three plates—with the exception of one, which consists of five—
their weights varying from 1·411 to 3·707 kilogrammes. Sixteen of
Van Wetteren's simple magnets gave, on the average, A=0·89, and
B=0·89, the weights being expressed in kilogrammes, and the di-
mensions in centims.; the other ten simple magnets gave A=0·88,
and B=0·88; and the ten compound magnets gave A=0·88,
B=0·85. These thirty-six magnets, simple and compound, render
it probable that I. is the general empiric formula for the state of
supersaturation; in like manner, II. is the probable formula for
the state of permanence of simple magnets, while for them A=B.
The deviation of the value of B for the compound magnets seems
to me proved by the difficulties I experienced in the determination
of the position of the maxima of the intensity-curve. We shall
thus have

$$P = A \cdot O \sqrt{s} \sqrt{\frac{l}{\sqrt{s}}},$$

a formula to which, for simple magnets, the following is to be added,

$$p = P \left(\frac{L}{l} \right)^{\frac{4}{3}}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(III.)}$$

as I have said above; while new observations will probably extend
formula III. to the compound magnets, which till then will be cha-
racterized by I. and II.

I have already investigated the moments of free magnetism of
several of these simple magnets, in the open as well as in the
closed state of supersaturation and of permanence, by the devia-
tions they produce in a compass-needle. By supposing that the
tangents of these deviations, for one and the same magnet placed
at the same distance from the compass, are nearly proportional to
these moments, since the lever-arms of the moments are nearly
equal, I have obtained by these tangents an approximate measure
of the free magnetism in these three cases—that is to say, of the
integral of the intensity-curve (1) for the magnet open, (2) for
the magnet closed and supersaturated, and (3) for the closed
magnet reduced to permanence.

In general the free magnetism for the magnet closed and super-
saturated is less than for the magnet closed and permanent; the
free magnetism, in the closed and permanent state, is to the
magnetism in the open state nearly as 3 to 10, a proportion which
becomes more and more constant as the magnet approaches defini-
tive permanence by successive separations of the armature. This
slowly progressive march toward definitive permanence, announced
by a slight rise of the given proportion, can be traced in another

manner by the deviations produced by the open magnet, which slowly diminish. The portative force is a datum too coarse for judging of these changes.

The highest value of the constant C of Bernoulli shown by my Tables is 22·4 ; I am convinced that to seek the manufacture of a magnet for which C should have a value 27 or 28 would be like searching after the philosopher's stone.—*Comptes Rendus de l'Académie des Sciences*, Nov. 27, 1876, pp. 1017–1020.

EXPERIMENTS ON THE SYMPATHETIC RESONANCE OF TUNING FORKS. BY ROBERT SPICE.

It is well known that a pair of forks having a vibration-number of 256 (Kœnig's Ut^3 forks), show the phenomenon of sympathetic resonance at distances apart varying from three to six feet. Beyond six feet, special and delicate means have to be employed to exhibit their resonance.

It is also well known that a pair of forks having a vibration number of 512 (Ut^4 forks), exhibit the phenomenon with similar intensity at far greater distances. The accepted solution of this difference of deportment is, that as in the latter case double the number of impulses are delivered in a second, consequently double the energy is conveyed to the distant fork.

If this explanation be sufficient, the following result should follow :—Forces radiating from a centre obey the law of inverse squares ; hence, if the amount of motion (or force ?) received by an Ut^3 fork at a distance of six feet from its excited fellow be represented by n, then (assuming an Ut^4 fork to have double the energy of an Ut^3 fork) clearly the amount of motion received by an Ut^4 fork at a distance of twelve feet from its excited fellow, should be represented by $\frac{n}{2}$. But so far is this from being the case, that the intensity instead of being one-half (as calculated), is more than double. In fact, at twenty feet the intensity of resonance of Ut^4 forks is undoubtedly greater than the intensity of Ut^3 forks at six feet.

A pair of forks were cast in a kind of bell-metal, and tuned to Ut^3. On Kœnig's boxes the resonance was quite obvious at twenty feet, and at forty feet the responding fork drove a cork ball of 8^{mm} diameter a distance of 10^{mm} ! This result was greater than that obtained with the steel Ut^4 forks of Kœnig. In view of these facts, it seemed to me that a different explanation was required to clear up the difficulty ; and, after a careful experimental examination of the question, I offer the following hypothesis :—

The intensity of sympathetic resonance of forks on their cases increases with the angular deviation or motion of the prongs.

The question of number of vibrations per second has its proper value ; but this value is small compared with the element above stated.

I proceed to explain this hypothesis. Suppose that we wish to set a pendulum in motion, but are required to fulfil the two following conditions :—first, we are obliged to hold the cord of the pendulum (point of suspension) in our hand, and this hand is also to be the motive power, to start and keep the pendulum in motion ; second, we are only to be allowed a lateral movement of the hand of one inch each way, making in all two inches.

Now the amount of motion or amplitude of a pendulum is estimated by the angle the cord or rod makes with the vertical ; and clearly, if the point of suspension moves laterally, it thereby creates an angle. If, further, the point of suspension has a re-ciprocal motion in accord with the possible time of the pendulum, then, by the principle of the summation of impulses, the motion of the entire pendulum will be gradually augmented up to a limit determined by well-known mechanical theorems. But if amplitude is expressed by angular magnitude, then, if the initial angle be in-creased, the total motion must be acquired in less time and be greater. From which it follows that, retaining the conditions above stated, if we operated on a pendulum ten inches long, we should set it in its maximum motion in less time and with less ex-penditure of force than if we operated on a pendulum fifty inches long. Experience confirms this.

A fork vibrates after the manner of a pendulum, and may be looked upon as an inverted pendulum ; but whereas, the *length* of a pendulum determines its vibrating period, the *length* and *thickness* together determine the period of a fork. Again, the period of a fork varies *directly* as the *thickness*, but inversely as the *square of the length* ; hence a small alteration of length will make a large difference in its period ; or, conversely, a large alteration of period does not imply a large difference in length.

From measurements made with an electro-chemical registering apparatus, which I designed for this and similar investigations, I find that when a fork of the usual dimensions (between Ut^3 and Ut^4) is in vibration, its stem or handle alternately rises and falls in accord with the period of the fork, through a distance of about $\frac{1}{80}$ inch. When a fork on its case is influenced by a distant fork, the case gives the stem this up-and-down motion, which is con-veyed to the prongs, and sets them in vibration, after the manner of the hand starting a pendulum, as specified above.

This motion of $\frac{1}{80}$ inch may be looked upon as a constant, and corresponds to the two-inch motion of the hand in the illustration. If we decrease the length of the fork without altering the constant, we thereby allow of a greater initial angle, the result of which we have already noted ; it is the same as shortening the pendulum-cord. This much understood, we are in a position to explain the deportment of the bell-metal forks cited. The velocity of sound in bell-metal is much less than in steel ; hence, retaining similar thicknesses in both cases, an Ut^3 fork in bell-metal would be shorter than an Ut^3 fork in steel, the ratio of the length of the steel to that of the bell-metal ranging as $90 : 75$. Therefore, though we

retain the vibration-number, we gain advantage from the shortness of the fork, and hence from the increase of angular motion of the prongs.

It was suggested to me that possibly bell-metal had the property of accepting motion more readily than steel. To test this point I made a pair of Ut³ steel forks, shorter than Kœnig's, and of course thinner, in order to retain the vibration-number. These forks behaved just like the bell-metal forks. Further, I made a pair of Ut⁴ forks as long as Kœnig's Ut³ forks, and of course thicker. These behaved like Kœnig's Ut³ forks. Finally, taking a Kœnig Ut³ fork on its case, and one of the short Ut³ forks also on its case; on placing them twenty feet apart, it was found that, on exciting Kœnig's fork, my short fork responded well, whereas on exciting the short fork Kœnig's did not respond at all.—Silliman's *American Journal*, December 1876.

NOTE ON THE CORRECTION OF THE VARIATIONS OF RATE OF ASTRONOMIC PENDULUMS PROCEEDING FROM DIFFERENCES OF ATMOSPHERIC PRESSURE.   BY A. REDIER.

Experiments, carefully made, at Brussels, Berlin, and London have proved that a seconds-pendulum varies 0·6 of a second per day between the two extreme barometric pressures. A pendulum perfectly regulated to 730 millims. pressure, would lose 0·6 second at 790 millims.

Mr. Airy has had constructed by Mr. Dent the first pendulum known which corrects these errors. In this construction a horse-shoe magnet, brought more or less near to an armature attached to the bob accelerates or retards the motion of the pendulum. The shifting of the magnet is effected by means of a float placed upon the siphon of a mercury-barometer. The result is perfect; but, in presence of the difficulties attendant upon the execution of this method, it appeared to me useful to seek out a more simple one.

I have tried, with some success, the effect of the friction of the air on the surface of the bob. An enclosed bob does not move at the same rate as one in free air; and I can with ease automatically augment or diminish the free space, according to the atmospheric circumstances; but this procedure, as well as Mr. Airy's, is again too complicated. There is a simpler and surer means of arriving at the result sought.

Suppose attached horizontally to the top of the bob the barometric case of an aneroid. The bottom of the case thus fixed faces the suspension. Upon the opposite flexible wall, at its centre, is sealed a mass, which rises or falls like the barometric case. When the atmospheric pressure increases, the mass approaches towards the suspension of the pendulum; and this goes faster, thus compensating the effect itself of that pressure. If the pressure diminishes, the reverse effect is produced. The motions of the case being known, it only remains to calculate the value of the movable mass as a function of the weight of the bob and of the position of the mass in relation to the bob.—*Comptes Rendus de l'Académie des Sciences*, Dec. 11, 1876, p. 1174.

XII. *A New Method of studying the Relation between the Viscosity and Temperature of Gases. By* SILAS W. HOLMAN, *Graduate of the Massachusetts Institute of Technology, Boston, Mass., U. S. A.*[*]

THE importance of accurate experimental data on the laws of gases, in connexion with the recently developed mathematical hypotheses of Clausius, Maxwell, and others, has led to much valuable research. It is hoped that the following method, with the preliminary results of the present paper, may prove to be a contribution to the precise knowledge of this subject.

According to the "kinetic theory," each molecule of a gas is constantly in rectilinear motion, possessing thus, in virtue of its mass, a certain momentum. Hence, if we have two layers of a gas moving over each other, we shall have a mutual interchange of momentum, arising from the transference of molecules from one layer to another, the result being a tendency towards an equalization of the velocities of the two layers. Thus is produced the effect of friction between the two layers. The amount of this in any particular case determines the viscosity or internal friction of the gas, and is expressed by the coefficient of viscosity η, which is represented[†] by the formula

$$\eta = \frac{Mu}{4\pi s^2}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

[*] Abstract of a paper read before the American Academy of Arts and Sciences, June 14, 1876. Communicated by the Author.

[†] Phil. Mag. [IV.] vol. xix. pp. 19, 434; vol. xx. p. 21.

where M is the mass of a molecule, u the "velocity of mean square" of the molecules, and s the distance between the centres of two molecules at impact. This formula, if true, shows that the viscosity of any gas is independent of the pressure upon it at a constant temperature, and increases proportionally to the value of u, i. e. to the $\frac{1}{2}$ power of the absolute temperature. Maxwell, however, has shown[*] that the assumption with regard to the nature of the impact determines the value of the exponent in this expression. From the experimental investigators of the subject, this exponent, x, has received the various values $x = 1, \frac{3}{4}, \frac{2}{3}, \frac{3}{4}$; but no results which I have yet seen furnish any accurate determination of the law.

The following method, with which I have as yet made some experiments on air only, is designed for the study of the variation of the viscosity with the temperature of gases, and, although capable of being used to furnish measurements in absolute units of the coefficient, has been arranged for differential effects only, for which it possesses special advantages.

Two glass capillary tubes, A B and C D, were placed side by side, each in a tin trough to contain a bath to regulate the temperature of the gas. Air-tight glass and rubber connectors extended from G to the gauge F, and to the end D of one ca-

pillary. The ends B and C of the capillaries were connected with the gauge E by means of a ⊤ joint of glass. The end A of the second tube communicated with the external air through the chloride-of-calcium tubes H and I. The size of the connectors at the ends of A B and C D was sufficient to allow the gas to assume the temperature of the bath. The tube at G was connected with a large flask, from which the air was continuously exhausted by means of a Richards's jet-aspirator. The size of this flask rendered the pressure constant in spite of
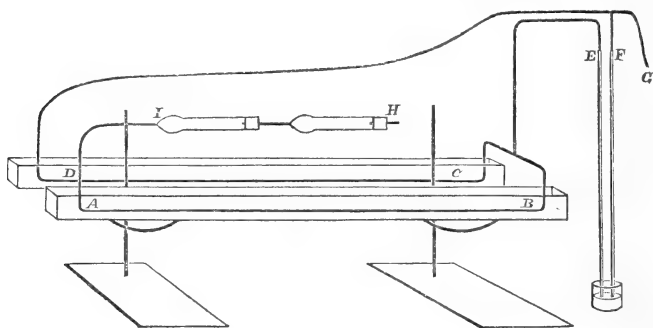
* Phil. Mag. [IV.] vol. xxxv. p. 211.

slight variations in water-pressure. An inspection of this arrangement will show that when the flask is exhausted and a vacuum produced at G, the air will enter at A under the atmospheric pressure, and will pass with constantly diminishing pressure to G; so that at any intermediate point, as the junction of the two tubes at B C, we shall have a pressure intermediate between the two extremes. It will also be seen that the same volume of air is successively transpired through A B and C D, provided that there be no leak, which was carefully guarded against by making all the joints about C, B, and E (which were the only ones that affected the results) as tight as possible. By the two baths, we may have the gas transpired successively through A B and C D either at the same or at different temperatures. Now if we denote by V_1, R_1, λ_1, η_1, &c. the volume of gas transpired by A B, the radius and length of A B, and the coefficient of viscosity of the air passing through it, while V_2 &c. represent the same quantities for C D—also if p_1, p_2, p_3 represent the pressure of the gas at A, B, C, and D respectively as obtained from the gauge and barometer-readings, then we may write[*]

$$V_1 = \frac{\pi R_1^4 t_1}{8\eta_1\lambda_1} \cdot \frac{p_1^2 - p_2^2}{2p} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$V_2 = \frac{\pi R_2^4 t_2}{8\eta_2\lambda_2} \cdot \frac{p_2^2 - p_3^2}{2p}. \quad . \quad . \quad . \quad . \quad (3)$$

But if both baths are at the same temperature $V_1 = V_2$ if $t_1 = t_2$, and $\eta_1 = \eta_2$, whence we may write

$$\frac{R_1^4\lambda_2}{R_2^4\lambda_1} = \frac{p_2^2 - p_3^2}{p_1^2 - p_2^2}. \quad . \quad . \quad . \quad . \quad (4)$$

Also in general it will be seen from the nature of the apparatus that $\dfrac{V_1}{1 + \alpha\delta_1} = \dfrac{V_2}{1 + \alpha\delta_2}$, where δ_1 and δ_2 represent respectively the temperatures at which V_1 and V_2 are transpired. Hence

$$\frac{\eta_1}{\eta_2} = \frac{R_1^4\lambda_2}{R_2^4\lambda_1} \cdot \frac{p_1^2 - p_2^2}{p_2^2 - p_3^2} \cdot \frac{1 + a\delta_2}{1 + a\delta_1}. \quad . \quad . \quad . \quad (5)$$

From equation (5) it will be seen that, in order to determine with this apparatus the ratio $\eta_1 : \eta_2$ between the coefficients of viscosity in the two tubes when the temperatures of these are δ_1 and δ_2 respectively, we have only to know the ratio of the dimensions as expressed by $\dfrac{R_1^4\lambda_2}{R_2^4\lambda_1}$, and to measure p_1, p_2,

* Pogg. *Ann.* vol. cxxvii. pp. 199, 353.

and p_3 by reading three mercury columns. Also we can obtain a value of $\dfrac{R_1^4 \lambda_2}{R_2^4 \lambda_1}$ from readings of the gauges when $\delta_1 = \delta_2$, which needs only to be corrected for expansion of the glass to be used directly in equation (5). The whole process is thus reduced to the simple matter of reading columns of mercury, no measurements of volumes of gas being necessary. The nature of the correction of R and λ for temperature appears by putting into the above formulæ, in which these values are supposed to be for $0°$ C., the coefficients of expansion of the glass $= A$; we thus get from (5),

$$\frac{\eta_1}{\eta_2} = \frac{R_1^4(1+A\delta_1)^4 \lambda_2(1+A\delta_2)}{R_2^4(1+A\delta_2)^4 \lambda_1(1+A\delta_1)} \cdot \frac{p_1^2 - p_2^2}{p_2^2 - p_3^2} \cdot \frac{1+a\delta_2}{1+a\delta_1}$$

$$= \frac{R_1^4(1+A\delta_1)^3 \lambda_2}{R_2^4(1+A\delta_2)^3 \lambda_1} \cdot \frac{p_1^2 - p_2^2}{p_2^2 - p_3^2} \cdot \frac{1+a\delta_2}{1+a\delta_1}. \quad . \quad . \quad (6)$$

Lest, however, an error might occur in the last reduction from a difference between the coefficient of expansion of the bore of a capillary tube and of its lineal expansion, I have carefully measured both, and find that the coefficient for the bore is 0.0000075, while for the linear expansion I find 0.0000080 per degree Centigrade, a difference too slight to affect the results in my use of it; I have thought it best to use the value 0.0000075, as it entered in the fourth power, while the other entered only in the first power. The tubes used have also been calibrated to ensure the selection of those of uniform bore; and their dimensions have been accurately measured by mercury and a micrometer-screw. The dimensions of the two tubes used in the experiments to be described were, for tube No. I., $\lambda = 1272.3$ millims., $R = 0.1098$ millim.; for tube No. II., $\lambda = 1274.1$ millims., $R = 0.1115$ millim.

To make an experiment with this apparatus, it is merely necessary to start the jet of water and allow the exhaustion to proceed until the mercury columns in F and E have come completely to rest. Readings are then taken of the heights of these columns, by means of a cathetometer, from a steel scale placed beside the gauges. The reading of the barometer corrected for instrumental error gives the pressure at A. All these are reduced to the freezing-point; and E and F are corrected for capillarity by the Tables of Delcros. The temperature of the baths is also taken by thermometers in various positions in the troughs. This must be kept constant throughout the experiment; and I have therefore principally used the temperatures of melting ice and boiling water. In the experi-

ments of which the following Table gives the results, advantage has been taken of the four methods of checking the results of one experiment by another, by reversing the direction of flow of the air through the tubes, and heating alternately, in each case, first one and then the other trough. In the Table, the first column gives the number of the experiment; column second the direction of flow of the air (which entered at the tube whose number is first given, and passed out from the other); columns three, four, and five give the pressures at A, B, and D respectively; columns six and seven show the temperatures, in Centigrade degrees, of the baths around tubes I. and II. respectively; column eight shows the values of the ratio $\dfrac{R_1^4 \lambda_2}{R_2^4 \lambda_1}$ at different temperatures; column nine the values of $\dfrac{\eta_1}{\eta_2}$, *i. e.* of η at the higher to η at the lower temperature; column ten shows the values of the exponent x in the equation $\eta = \tau^x$. This is the quantity which it was the object of the experiments to obtain.

No.	Direction.	p_1.	p_2.	p_3.	T_I.	T_{II}.	$\dfrac{R_1^4 \lambda_2}{R_2^4 \lambda_1}$.	$\dfrac{\eta_1}{\eta_2}$	x.
		millim.	millim.	millim.	°	°			
1.	I.–II.	759·9	525·2	16·3	17·0	17·0	0·912		
2.	,,		549·3	17·1	17·0	47·5	...	1·083	0·799
4.	,,	759·8	525·6	18·0	15·1	15·1	0·916		
5.	,,		584·4	18·9	,,	,,	0·921		
6.	,,	765·7	550·9	18·6	17·8	17·8	0·934		
7.	II.–I.	,,	490·7	17·7	17·5	99·0	...	1·212	0·776
8.	,,	,,	491·2	17·6	17·5	99·5	...	1·206	0·755
9.	,,	,,	490·0	17·3	17·5	99·8	...	1·215	0·780
11.	,,	755·2	467·8	20·4	0·0	100·0	...	1·272	0·771
12.	,,	,,	468·4	19·4	,,	,,	...	1·267	0·757
13.	,,	,,	467·9	19·6	,,	,,	...	1·271	0·768
14.	,,	,,	467·7	19·3	,,	,,	...	1·273	0·773
16.	,,	,,	544·2	20·7	0·0	0·0	0·927		
17.	I.–II.	756·7	525·3	23·4	,,	,,	0·928		
18.	,,		594·8	21·5	0·0	100·0	...	1·277	0·782
19.	,,	761·4	529·1	16·1	100·0	100·0	0·933		
20.	,,	762·0	530·2	16·7	,,	,,	0·937		
21.	,,	763·1	452·2	18·5	100·0	0·0	...	1·259	0·738

In the calculation of the ratio $\dfrac{\eta_1}{\eta_2}$ of this Table, the value of $\dfrac{R_1^4 \lambda_2}{R_2^4 \lambda_1}$ used was the mean of that obtained from experiments 16 and 17, after correcting for temperature. The agreement of these two values within 0·1 per cent. is a test of the accuracy of the method, as the two experiments were made on different

days, and the direction of the current was reversed. It will be seen that the value of this quantity increases slightly with the temperature, as we should expect from the slight difference in size of the two tubes used. The values of x will be seen to agree quite closely, with the exception of experiments 2 and 21.

A comparison of these results with those of Meyer, Maxwell, Puluj, or von Obermayer will show the superior accuracy of this method. Such a comparison can be most easily made by means of a graphical construction. Let $\eta = c\tau^x$ be the general form of the equation ; then

$$\log \eta = \log c + x \log \tau,$$

which is of the form of the equation to a straight line referred to rectangular axes, and making an angle whose tangent is x with the axis of X, the value of $\log c$ being the intercept on the axis of Y. Therefore, if we plot the various values of $\log \eta$ as ordinates, and of $\log \tau (\tau_0 = -273°$ C.) as abscissæ, we shall obtain points lying along a straight line, from whose tangent with X the value of x may be determined. An inspection of the lines thus obtained from the data of various experimenters furnishes the most ready means of comparing the accuracy of their results. By such an examination it will be seen that, while Meyer obtained values of x from $x = 2·3$ to $x = 0·21$, and Puluj from $x = 0·65$ to $x = 0·47$, the above Table shows variations from $x = 0·799$ to $x = 0·738$ only in these preliminary experiments.

As a result, then, of these experiments, it would appear that the viscosity of air increases proportionally to the $0·77$ power, nearly, of the absolute temperature between 0° and 100° C. But more determinations at temperatures between these limits are necessary to prove the law of this variation.

XIII. *On the Fixed Lines in the Ultra-red Invisible Region of the Spectrum.* By JOHN WILLIAM DRAPER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I DESIRE to call the attention of those experimenters who are at present occupied in investigating the less-refrangible end of the spectrum, to a paper illustrated by an engraving in the Philosophical Magazine for May 1843. From this it will be seen that in the preceding year I had made photographs, not only of the Fraunhofer lines, but also of many others at both ends of the spectrum, and in exploring the less-refrangible region had found three great lines far

beyond the line A, and had designated them as α, β, γ. Of the existence of a fourth, still lower down, I had obtained imperfect evidence.

Three years subsequently these lines were rediscovered by MM. Foucault and Fizeau, who used the photographic method previously discovered by me. In 1871 they were again detected by M. Lamansky by the aid of a thermo-multiplier.

I formerly supposed that the experiments of Sir John Herschel, made with paper blackened on one side and washed with alcohol on the other, indicated the existence of these lines; but a more attentive consideration of the apparatus he employed has led me to change that opinion. He did not use a slit, but the direct image of the sun, which with the optical train he had was more than a quarter of an inch in diameter. Under such circumstances it was impossible that either these or any other lines could be seen. The result he obtained was a succession of circular patches or spots—solar images—commencing above the yellow, and continuing into the ultra-red.

More recently Captain Abney has experimented in the same direction, using collodion containing various colouring or other material supposed to promote the photographic action of the less-refrangible rays; and in a very recent Number of Poggendorff's *Annalen* (No. 10, 1876), MM. H. C. Vogel and O. Lohse have published similar experiments. It is this last paper that leads me to make the present remarks; for those physicists seem not to be aware that what they are attempting now was accomplished in America thirty-five years ago.

I think, from some expressions that Captain Abney has used in one of his papers, that he entertains a very low estimate of the photographs so produced; he depreciates the process by which they were obtained very much. Sir John Herschel, than whom no one was a more competent judge of a fine photograph, says of one of these that I sent him (Philosophical Magazine, February 1843), "I should hardly be doing justice to the beauty of the specimen itself as a joint work of nature and art were I to forbear acknowledging its arrival, and offering a few remarks on it..... The spectrum itself is extremely remarkable and beautiful..... Want of habitude in the manipulation of the daguerreotype process, and by no means want of sun, prevented my obtaining any thing like so fine an impression." If Captain Abney will for once excuse an inventor for praising his own invention, I who have seen very many photographs, and know the difference between a good and an imperfect one, will assure him that

these spectrum-impressions were superb.   If he will only try the process, he will never give himself any concern about collodion spectra again.

I have attempted ineffectually to draw attention to this process.   There is, in my opinion, no fact more striking among the chemical effects of light, none that promises, from its investigation, more important results.

There are two modes by which this process can be carried into effect.

1st. Submit a silvered plate to the vapour of iodine until it has acquired a yellow tarnish; or, better (since the plate will become thirty times more sensitive), submit it to iodine, bromine, and again to iodine, until the same tint of tarnish has been obtained.

Now expose it to a pure spectrum in a room to which a feeble daylight is admitted.   On developing by the vapour of mercury, a photograph will be obtained of the visible spectrum from end to end, and extensive regions beyond the violet and the red respectively.   In all the part above the blue the daylight and the sunlight have acted in unison, in all that below the blue they have antagonized, and the plate remains unacted upon, except where the Fraunhofer lines occur, and where, therefore, there has been no sunlight.   Then the daylight has depicted those lines in white, while the more refrangible are black.

2nd. Prepare a plate as before.   Expose it to a feeble daylight or lamp-light, until, if developed with mercury, it would whiten all over.   But instead of developing it, now let it receive a pure spectrum.   Then develop, and the result will be the same as in the preceding case.

So it is not necessary that the daylight and the sunlight should act simultaneously; they may act successively —an important fact in settling the nature of their antagonism.

To produce a perfect result, the two (the daylight and the sunlight) must be exactly balanced.   If the daylight should preponderate, the protection is only in the extreme red; as it is diminished the protection extends higher and higher; and the exact equipoise being attained, it reaches the confines of the blue.   All the Fraunhofer lines in the less-refrangible portion of the spectrum come out in white; all those in the more refrangible are dark.   In my early experiments I could not obtain D, E, F; but my son, Henry Draper, operating under this rule, has since photographed them all.

Of all photographic facts this antagonizing action is the most extraordinary.   I still work at its elucidation, though in

a very desultory manner. I earnestly commend it to the attention of those interested in an examination of the chemical action of radiations, as one of the most important and promising topics.

JOHN W. DRAPER.

University, New York,
December 13, 1876.

XIV. *Contributions to the Theory of Luminous Flames.*— Part II. *By* Dr. KARL HEUMANN [*].

Influence of Withdrawal of Heat from, and Addition of Heat to Luminous Flames.

A LTHOUGH the phenomenon of a small distance existing between flame and burner, or flame and a cold substance placed therein, is most apparent in the case of non-luminous flames, or of those flames which have been diluted by indifferent gases, nevertheless this appearance is also noticeable in the case of luminous flames. In the latter flames the eye is somewhat overpowered by the light, and hence has difficulty in observing the vacant space; the recognition of this space is made easier by placing a screen in such a position as to cover the luminous part of the flame as completely as possible.

Mention has already been made of the fact that a flame loses luminosity by being pressed down or widened out by the introduction of a cold substance; and by properly regulating the experiment, it has been shown that the withdrawal of heat is *of itself* sufficient to account for the observed diminution in luminosity. By combining the results so obtained with those which we have gained concerning the distance between flame and burner, or flame and cold object placed therein, we are led to deductions of great practical interest.

If a cold metallic wire be placed in a luminous gas- or candle-flame, the flame is totally extinguished in the immediate neighbourhood of the wire, and the luminosity of the flame is diminished throughout a very considerable area.

In this experiment the low conductivity for heat of gases comes into play, aided by the great freedom of motion and diffusibility of the particles, whereby highly heated particles are continually brought into fresh contact with the cold wire. The cooling action of the wire is therefore the greater, and extends throughout a larger space, the lower the temperature of the wire itself. The wire is therefore also more potent in

[*] Translated from Liebig's *Annalen*, vol. clxxxiii. part 1, pp. 102-141, by M. M. Pattison Muir, the Owens College, Manchester.

this respect immediately it is introduced into the flame ; as its own temperature increases, it loses its power of cooling the surrounding particles of heated gas. A thick wire also will evidently cause a diminution of luminosity for a longer period than a thin wire.

It is generally a matter of indifference in what part of the flame the cooling body is placed ; hence the *burner* itself may play a not unimportant part in cooling the burning gas in its neighbourhood, and so in aiding in the production of the dark zone of flame which is noticed just over the orifice of the burner.

Before studying the action of the burner in detail, however, it will be well that we should inquire more narrowly into the cooling action exercised by a cold object brought into the flame upon the luminosity of that flame. If a luminous gas-flame be *slightly* pressed down by means of a porcelain basin, the luminosity of the flame is somewhat diminished, and the basin is covered with a deposit of soot. If, however, the basin be at the beginning of the experiment *deeply* depressed within the flame, the luminosity is at once decreased ; but *there is no deposit of soot on the basin*.

I have already shown that the decrease of luminosity is to be traced in this experiment to the cooling action of the cold object placed in the flame. If, as may easily be done, an equal area of the porcelain basin be brought into the flame in each case, the cooling action of this cold surface will be approximately the same in each* ; nevertheless when the basin is brought into the upper part of the flame it is covered with soot, while no such deposition occurs when the basin is brought into the lower part of the flame.

The following experiments will throw light upon these phenomena.

A porcelain rod was brought into the lower part of a gasflame burning at a round orifice 8 millims. in width ; the flame throughout a considerable area became blue. (By cutting off the still luminous portion of the flame by means of a shade, the action of the porcelain rod is rendered more apparent.) No soot was deposited on the porcelain. The same rod was held in the upper part of the luminous flame: it was gradually covered with a tolerably thick deposit of soot ; at the same time diminution in the luminosity of the flame was noticed.

In order to explain these facts, I put forward the following

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* Rather more heat will be withdrawn from the upper hot flame, on account of the greater difference of temperature between it and the basin, than from the lower part of the flame, in equal time-intervals.

hypothesis (I shall endeavour to bring together further proof of this hypothesis in the succeeding parts of the present paper):—

Carbon-containing sources of light may burn with luminous flames, i. e. *with separation of carbon in the flame, or with non-luminous flames,* i. e. *without separation of carbon*. The maintenance of a certain (high) temperature, dependent upon the nature of the combustible substance, is an essential condition of luminosity; a flame whose temperature has been lowered by any means is no longer able to bring about the required separation of carbon. Combustible matter, when diluted with indifferent gases, requires to be maintained at a higher temperature in order that it may burn with a luminous flame than when it is undiluted with such gases.*

Chemistry furnishes us with many reactions analogous to the last-mentioned circumstance : thus dilute solutions generally undergo decomposition or throw down precipitates only when strongly heated, &c.

The various parts of a luminous flame are possessed of very varying temperatures ; a cold object brought into different parts of the flame, and withdrawing nearly equal amounts of heat from each, will cool the upper hot parts only slightly below that point at which deposition of carbon takes place ; this process will therefore continue all around the cold object. In such a case as this, separation of carbon will also be taking place at a point in the flame lower than that where the cold object is situated ; hence these little particles of separated carbon will rise upwards, and some of them will be deposited on the cold object. But if this object be placed in the lower and cooler part of the flame, the temperature of this part of the flame will be decreased beyond that point at which carbon is separated ; hence the object will not be covered with a layer of soot : and, from the conditions of the experiment, there can be no separation of carbon in parts of the flame underneath the point where the cold object is now placed.

Bearing these points in mind, we shall be able to trace the phenomena noticed in the first part of the experiment with the porcelain rod to two circumstances :—

1. Deposition of carbon was noticed upon the rod when held in the upper part of the flame, because the cooling action of the porcelain was not sufficient to reduce the temperature of this part of the flame below the point at which carbon is separated.

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* I strenuously uphold the old idea that the luminosity of carbon-containing flames is to be traced to the presence of separated carbon. In a future paper I will bring forward new proofs of the truth of this idea.

2. The particles of carbon separated in the lower uncooled parts of the flame rose upwards, and were deposited upon the porcelain rod.

The decrease in luminosity, unaccompanied by deposition of carbon, which was noticed when the rod was much depressed in the flame, is to be traced to the fact that the temperature necessary for separation of carbon could not be attained in this part of the flame, which previous to the experiment was already cooler than the upper part.

These considerations teach us that, in order to obtain a large deposit of carbon (as in the manufacture of lampblack &c.), the cooling of the flame should not be carried too far, while at the same time the maintenance of a very high temperature or the presence of much air should be avoided. Further, we learn that, *in order to obtain deposition of carbon, it is not necessary to cool the flame,* inasmuch as the maintenance of a high temperature is a necessary condition of the separation of carbon. The object brought into the flame may be compared to a redoubt which intercepts the balls shot forth. The cooling action exercised is productive of carbon-deposition only so far as it prevents the oxidation both of the particles of carbon suspended in the flame and of those caught by the extended surface.

Hence it follows that the surface of a glowing body immersed in the luminous flame must become covered with soot.

The carbon which is separated in a smokeless luminous flame is burned partly in the outer, partly in the inner portions of the flame : it is evident, however, that this burning will take place to a less extent where the flame is enclosed by the solid body placed within it than where it is surrounded by air. Even in the former part of the flame, however, the supply of air is not altogether shut off ; hence a part of the soot which has been deposited, and which is at a high temperature, will be burned. Deposition of soot is therefore generally somewhat less upon a hot than upon a cooler surface ; nevertheless it may happen that a very cold substance is scarcely, if at all, covered with soot.

In performing the following experiment, which exhibits the fact that a hot surface may have carbon deposited upon it, care must be taken not to bring the surface covered with carbon into the air until it cools ; else the soot will almost immediately be burned. A platinum basin is placed in a vertical position; the concave side is heated by means of a Bunsen's lamp placed horizontally ; and a luminous flame burning at the orifice of a tube is caused to play against the convex side of the basin. A deposit of soot is soon noticed. By moving

the luminous flame slightly to one side, the deposited soot may be caused to burn in the air.

By allowing a somewhat thick layer of soot to be formed, and by then simultaneously removing both flames, it is possible to obtain a considerable quantity of unburned soot when the basin has become cold.

As confirmation of the statement that the cooling action of a substance held in the flame is only of secondary importance so far as regards the deposition of carbon upon that substance, and that the main action consists in intercepting the upgoing particles of carbon which have been separated in the lower parts of the flame, the experiment with the porcelain rod may be repeated. If the rod be carefully watched, it is seen that soot is at first deposited only upon the under surfaces of the rod (that is, upon those surfaces which are opposed to the uprising current of gas), and that the deposit spreads to the upper surfaces only after some time has elapsed, and even then that this latter deposit is much thinner than the former.

These facts furnish further and most weighty arguments in favour of the generally adopted view that the soot actually exists in the flame in the form of a *solid body*, and not in the gaseous state as Frankland supposes. For if the latter supposition were true, deposition of soot must be a consequence of withdrawal of heat, and condensation of the thick light-giving hydrocarbons would take place upon the upper surfaces of cold bodies placed in the flame.

Aided by knowledge gained from these experiments, we shall be in a position the better to understand the action of the burner as a means whereby heat is withdrawn and the flame rendered less luminous, and also the action of the cold inrushing gas. Just as a metallic rod placed near to a burner whence a flame is issuing causes a somewhat extended dark spot in the latter, so it must be admitted that the cold top of the burner itself will exercise a similar action upon the non-luminous part of the flame which always immediately surmounts it. In this case a second factor, viz. the action of the cold inrushing gas, also comes into play.

The action of the burner tending to decrease luminosity is naturally more marked in the case of small than of large flames, because the whole area of the flame is less, and also because the temperature throughout that area is less, in the former than in the latter cases. By causing the flame of a bat's-wing burner to become very small, the cooling action of the burner may succeed in entirely extinguishing the flame.

The cooling action of the burner becomes very marked when

the surface of the burner is large, and when the burner itself is made of thick metal : this is especially marked with burners in which the gas issues from a number of small holes pierced in a thick plate of iron.   If the supply of gas to such burners be small, the light effect is almost *nil*.   The heating effect of such (roasting) burners is also, I think, small compared with the consumption of gas.   If the supply of gas be increased in such burners, the tops of the flames become luminous, but the lower parts always remain blue.

If a burner causes decreased luminosity by acting as a withdrawer of heat from the flame, one would expect that burners made of material which is a good heat-conductor would exercise a greater action in this respect than those made of material having a lower conductivity for heat.   Nevertheless I am assured that in practice the only fault found with metallic burners is the readiness with which they undergo oxidation ; and a Commission by the English Board of Trade * has reported unfavourably as regards the use of porcelain or steatite burners, so far as increased luminosity is concerned.

The differences in luminosity obtained by the use of different burners may be so small as only to be distinguished by very careful experiments.   I found that the ordinary burners were of no use in performing comparable experiments, a small difference in the cavity of two burners, or in the position of the stopcock &c., often vitiating the experiment. I therefore had burners of different material specially prepared : these were exactly similar in construction, of the same size, and pierced with the same instrument with a cross-shaped opening ; they only differed in the material of which they were constructed.   These burners were attached to similar tubes ; and the light obtained from each under exactly the same conditions was tested by means of the photometer.

	Intensity of light, in candles.	Gas-consumption, in litres.
Steatite burner . .	0·9	36
Iron burner . . .	0·85	37

Besides the errors inherent in the photometrical methods, it was found that other errors rendered the results in the case of the iron burner of little value.   Within the first few minutes after lighting the gas, the intensity of the light, as also the consumption of gas, showed most remarkable variations. These were evidently to be traced to the heating of the burner, whereby the opening was decreased in size, and hence the velocity of the issuing gas was also decreased.   In order to

* *Polyt. Centralblatt*, 1872, p 138, &c.

remove this source of error, the tube to which the iron burner was attached was caused to pass through a piece of glass tubing closed at the bottom with a cork and at the top with a watch-glass, the top of the burner just appearing above the latter. This tube was filled with water, which was maintained at 15°. The experiments, the results of which are detailed, were conducted with every precaution, and each set under exactly similar conditions [*].

Steatite burner.				Iron burner.			
Intensity of light, in candles.	Mean.	Gas-consumption, in litres.	Mean.	Intensity of light, in candles.	Mean.	Gas-consumption, in litres.	Mean.
0·12 0·12	0·12	14·5 14·7	14·6	0·16 0·18 0·17	0·17	19·5 20·0 19·0 19·5	19·5
0·3 0·3	0·3	22 21 21	21·3	0·3 0·3 0·3	0·3	25·5 26·0 25·5	25·7
0·9 1·0 0·98	0·96	39 39 39	39	0·9 0·8 0·9	0·87	41 42 42	41·7

It was not considered advisable, on account of the arrangement of the testing-apparatus, to further increase the supply of gas.

From the second and third series of numbers it is at once apparent that the steatite is preferable to the iron burner, inasmuch as an equal intensity of light is obtained with a less consumption of gas. The first series of numbers does not, however, appear to bear out this result, inasmuch as the intensity of light obtained from the gas burning from the steatite burner is rather less than that obtained in the experiment with the iron burner for an equal gas-consumption.

If, however, the results obtained from the iron burner be compared among themselves, it will be seen that this increase of light does not always bear the same relation to increase of gas consumed. The reason for this is to be found in the fact that the cooling action of the burner is greater in the case of small than of large flames.

If the results be plotted in curves, the difference between the two burners is very apparent.

I therefore feel justified in contradicting the statement of the Board-of-Trade Commission, to the effect that the advantages of burners made of material of low conducting-power

[*] In the original paper the precautions are detailed.

for heat are "altogether imaginary." If the iron burner were not maintained at the initial temperature of the experiment by extraneous means, its action in withdrawing heat and so causing decreased luminosity, would of course be much less marked than it was in these experiments. Nevertheless I consider that these experiments prove the fact that *a notable diminution of light-intensity is brought about by the employment of metallic burners.*

The cooling action of a metallic burner may be observed in another and very instructive manner. If an ordinary burner be carefully examined shortly after the flame has been lighted, a small quantity of moisture containing tarry matter will be noticed collecting about the orifice of the burner. If the burner be artificially cooled, this phenomenon becomes more noticeable. If the burner be not cooled, the little drop of liquid is soon evaporated ; but a brown residue of tarry matter remains as testimony to the cooling action of the burner upon the issuing gas.

It has been already shown that a metallic wire, when held in a flame, causes a considerable diminution in the luminosity of the flame, but that this decrease does not take place when the wire is heated to redness. Hence we should infer that the action of a metallic burner in reducing luminosity would disappear if the burner were strongly heated. Unfortunately, in carrying out an experiment to prove the truth of this supposition, it does not appear possible to avoid raising the temperature of the gas itself; hence the increased luminosity cannot be altogether referred to the increased temperature of the burner.

In considering the phenomenon of the space which is maintained between flame and burner in the case of coal-gas burning under pressure, we have taken into consideration the cooling action exercised upon the flame by the entering cold gas. This cold gas would tend to decrease the luminosity of the flame. Let us suppose that the flame could exist for a short time independent of the propelling action of the gas ; the temperature of the flame would not then be uniform throughout ; by reason of the air diffusing inwards, and also because of conduction and radiation, the outer portions would part with more heat than the inner; the hottest part of the flame would therefore be at, or about, the centre. Suppose now that cold gas were suddenly admitted to the flame from beneath, it is evident that the greatest absorption of heat would take place in the lower parts of the flame, and that the hottest point would no longer be at the centre, but towards the upper extremity of the flame. At the same time every

part of the flame must participate in the cooling effected by the entering cold gas ; those portions whose temperature was already but very little above that at which separation of carbon occurs would be cooled below this point, and would therefore be rendered completely non-luminous, whereas the hotter portions of the flame would have their luminosity decreased, but not entirely removed. Hence it follows that the luminosity of the whole flame will be decreased the more the colder was the entering gas. The total luminosity, on the other hand, would be increased, if the cooling action of the entering gas were neutralized by raising this gas, before it was allowed to enter, to the temperature of the flame.

I had reached this point in my theoretical considerations, when I met with a paper on the increase of luminosity occasioned by heating the gas, by A. Vogel[*], and the Report of the English Board of Trade, already referred to, the results of which are contradictory to those of Vogel. Vogel conducted the gas through a V-tube placed successively in ice, freezing-mixtures, boiling water, and heated paraffine, and used a steatite burner attached to the V-tube. Representing the luminosity at $18°$ as 100, he found that at $0°$ this was reduced to 76 and 85, at $-20°$ to 33 and 44, at $+100°$ to 104, and at $+160°$ to 118. The influence of decreased temperature is here very marked ; that of increased temperature is somewhat less so.

The results of the Board-of-Trade experiments are directly opposed to those of Vogel ; they indicate no difference in luminosity for the temperatures of $0°$ and $145°$. So far as I know, Vogel has not answered these statements ; and although I have unfortunately been as yet prevented from repeating the experiments, I nevertheless venture to put forward the following suggestions as perhaps likely to throw light upon the subject when it is regarded from a general point of view.

And first as regards the experimental data in Vogel's paper. When we consider the small conductivity for heat of gas, it can scarcely be admitted that the gas which passed through the V-tube maintained at $0°$, $-20°$, $100°$, and $160°$ was really itself at these temperatures. A tube bent repeatedly might have been advantageously substituted for the V-tube, and a small thermometer inserted near the burner in the tube conducting the gas would have enabled the experimenter to read off the actual temperature of the gas just before it reached the burner.

In my own photometric observations I found it of the

[*] *Bayerisches Industrie- und Gewerbeblatt*, 1869, p. 124; Wagner's *Jahresbericht*, 1869.

utmost importance to determine the consumption of gas with great care. I do not think that the consumption would be the same in unit of time when the V-tube was kept cold as when it was heated ; and I do not doubt that had variations in the consumption been taken into account, the numbers obtained by Vogel would have been somewhat altered. These corrections, however, would not in any way invalidate the general conclusions which Vogel has drawn concerning the influence of temperature upon luminosity ; they would probably have only served to make more marked the difference between the numbers obtained. We must therefore seek elsewhere for some explanation of the great difference between Vogel's results and those obtained by the Commission already referred to. I have not been able to obtain any details of the experimental methods adopted by the Commission ; so that I am unable to say whether these were accurate in principle and were carried out with due care. One thing, however, appears to be essential in carrying out such experiments if the results are to be comparable ; and that is, to ensure that the composition of the gas is always the same. It is quite possible that two gases might give lights of equal intensity, for equal consumption, at $18°$, and that nevertheless the intensities might be very different at $0°$ or at $-20°$. A gas which owes its luminosity mainly to the presence of ethylene would give a very luminous flame at low temperatures, at which another gas, owing its luminous effect chiefly to the presence of *condensable* benzene &c., would yield a comparatively non-luminous flame. Hence it might be said that when Vogel obtained a marked decrease in luminosity by decreasing the temperature of the gas employed, the English Commission contradicted his results because they worked with a gas which was very poor in condensable hydrocarbons. The supposition of a difference in the composition of the gases used, however, will not serve to explain the difference in the results obtained at high temperatures. It is, to say the least, very doubtful whether any formation of condensed hydrocarbons could take place at $100°$ to $160°$. My own theoretical deductions pointed to the conclusion that the luminosity of a flame is increased if the cooling effect of the burner and of the entering gas be neutralized. The only way in which I can explain the discrepancies in the two sets of results, is by taking into account the want of delicacy inherent in the photometric methods. It cannot be astonishing to find that our photometers should fail to detect a difference in the intensity of light of a flame whose temperature is about $2000°$ when that temperature is further increased by $100°$ or $145°$. Were the gas passed through a tube main-

tained at a *red heat*, a marked increase in the luminosity of the flame produced by burning the gas would, I believe, be noticed.

I have already remarked that the cooling effect of the burner may be neutralized by heating the burner, and that the entering gas is hereby itself unavoidably raised in temperature. Inasmuch, then, as it is impossible to pass a heated stream of gas through a cold burner without raising the temperature of the latter, or to pass gas through a hot burner without increasing the temperature of the former, it is evident that in an experiment in which the gas is passed through a hot burner the effect obtained will be equal to the sum of these two causes. If the results of experiments are to be depended upon, every precaution must be taken, and especial care must be given to a determination of the quantity of gas consumed, so that, if a difference in luminosity be really observed when the tube of the burner is heated, it may be certainly known whether or not any change in the consumption of gas has occurred.

Blochmann has shown that the diminution noticed in the consumption of gas after the issuing stream is ignited is due to the action of the heated burner ; hence one would infer that if the gas be passed through a strongly heated tube, a considerable diminution in the quantity consumed will be observable. It was therefore most important that determinations of the quantity of gas consumed should be carried out simultaneously with determinations of the intensity of the light obtained by burning the gas. Such determinations have been carefully carried out by me, the gas being passed from a meter through a platinum tube which could be heated by means of a Bunsen's lamp.

Observations taken before heating the tube and after it had become cold gave the same result. Two platinum tubes were used, each 10 centims. in length, one being 4 millims., the other 8 millims. in width. These were heated so that they became red-hot to the very point whence the gas issued. The light radiated from the hot tubes was proved to be so small in amount as not to influence the results of the experiments. Inasmuch as the relation existing between intensity of light and consumption of gas is not as yet accurately known (the results of different observers differ very much), I have assumed that the calculated diminution in intensity of light is proportional to the observed diminution in consumption of gas. Thus, as 16-litre gas gave light equal to 0·4 candle, I have supposed that 15·5-litre gas would give light equal to 0·38 candle. The numbers thus obtained are given in column 7.

The differences between these numbers and those actually obtained represent the gain in intensity of light by heating the gas. This gain is stated in column 9 in per cents. calculated on an equal consumption of gas.

		1.	2.	3.	4.	5.
		Intensity of light, expressed in candles, according as the gas was passed through		Consumption of gas, in litres.		Increase in intensity of light, in per cents.
		Cold tubes.	Hot tubes.	Cold tubes.	Hot tubes.	
I.	narrow tubes.	0·4	0·6	16	15·5	50·0
II.		2·1	2·9	38	37·5	38·1
III.		5·6	6·1	83	82	8·9
IV.	wide tubes.	0·8	1·5	23	23	87·5
V.		2·0	2·7	41	41	35·0
VI.		4·9	7·6	123	121	55·1

		6.	7.	8.	9.
		Decrease in consumption of gas, in per cents.	Intensity of light for the decreased gas-consumption (column 4).		Gain in intensity of light, in per cents.
			Calculated.	Found.	
			Cold tubes.	Hot tubes.	
I.	narrow tubes	3·1	0·38	0·6	57·9
II.		1·3	2·0	2·9	45·0
III.		1·2	5·5	6·1	10·9
IV.	wide tubes	0	0·8	1·5	87·5
V.		0	2·0	2·7	35·0
VI.		1·6	4·8	7·6	58·3

These results indicate that a considerable increase in intensity of light is brought about in every case by heating the tube of the burner. This increase is much more marked in the case of small flames than in those produced by using large quantities of gas. The reasons for this latter fact are probably these : the cooling action of the burner is more marked in the case of small than of large flames ; and when large quantities of gas are passed through a heated tube, there is every likelihood that a portion of that gas only will be raised to the temperature of the tube. In the experiments in which the gas was passed through a wide platinum tube, it is noticed that the increase in intensity of light is very marked with a large consumption of gas. I account for this apparently contradictory result as follows. By comparing the numbers expressing the

intensity of light in experiments V. and VI. with those in the preceding experiments, it is seen that for equal consumption of gas the intensity of light is much smaller in the former than in the latter cases. The outer surface of the flame is too small in proportion to the total volume of gas to allow of such energetic action as is required for the production of a tempe-rature sufficiently high for the maintenance of great lumino-sity ; but inasmuch as a large volume of gas is present, artifi-cial raising of the temperature of that gas very greatly increases the luminosity of the flame: this effect, however, must be decreased by increasing the velocity of the issuing gas ; expe-riment V. shows that this is really the case. As the velocity of outflow increases, the increase in luminosity becomes much less in proportion to what it was at the beginning of the ex-periment, until a point is reached, after which the luminosity begins to decrease, the flame finally burning blue. If the gas be passed through a cold tube, this decreased luminosity accom-panying increased velocity of the issuing gas becomes much more marked ; hence after a certain point the effect of increased gas-consumption with a hot tube in increasing luminosity must again surpass that with a cold. This is seen by throwing the results of the Table into graphic form and comparing the curves representing the increase in luminosity for equal but increasing gas-consumption according as hot or cold tubes are employed. At first the curve of the hot tube keeps far ahead of that of the cold; as the consumption of gas increases, the curves become equal (when 40 or 45 litres of gas are con-sumed); but after this point the hot-tube curve again passes ahead of that of the cold tube.

Experiments I., II., and III. show that heating the tube through which the gas passes causes a decrease in the quan-tity of gas consumed. The decrease is more marked in the case of narrow than of wide tubes.

These experiments show that by considerably raising the temperature of the issuing gas and of the burner, a marked increase in the luminosity of the flame becomes apparent. The increase of temperature was too small in the experi-ments of the English Commission to allow of the effect being noticed.

I have also made experiments with flat flames, produced by allowing the gas to issue from a narrow platinum tube the end of which was hammered together so as to leave only a narrow slit. The following Table contains the results :—

	1.	2.	3.	4.	5.
	Intensity of light, in candles.		Gas-consumption, in litres.		Increase in light-intensity, in per cents.
	Cold tubes.	Hot tubes.	Cold tubes.	Hot tubes.	
VII.	1·3	2·2	38	35	69·2
VIII.	2·6	3·5	55	51·5	34·6
IX.	4·7	5·8	90	81	23·4

	6.	7.	8.	9.
	Decrease in gas-consumption, in per cents.	Light-intensity for decreased gas-consumption (column 4).		Gain in light-intensity, in per cents.
		Calculated.	Found.	
		Cold tubes.	Hot tubes.	
VII.	7·8	1·2	2·2	83·3
VIII.	6·3	2·4	3·5	45·8
IX.	10·0	4·2	5·8	38·0

From these results it is again apparent that heating the tube causes a marked increase in the luminosity of the flame, and that this increase is the greater the less gas is passed through the tube. The very considerable decrease in the quantity of gas consumed occasioned by heating the tube is most remarkable, and shows that when the gas makes its exit through a narrow opening the expansion caused by heating exercises a greater retarding effect upon the velocity of issue than when a wide exit-tube is employed.

In the first part of this paper, when describing the increase in luminosity brought about in a non-luminous flame of mixed coal-gas and carbon dioxide by heating the tube through which the mixture passed, I stated that this increase was no longer noticeable if the tube were heated at some distance from its orifice ; and I accounted for this fact by the cooling action exercised on the hot gas by that part of the tube the temperature of which had not been increased.

A similar action might be anticipated to occur in the cases just described, wherein heating the platinum tube which served as a burner caused an increase in luminosity. Experiments justify this anticipation, and show that the increase in luminosity is materially affected by the position of the source of heat employed for raising the temperature of the platinum tube. The following numbers were obtained :—

		Light-intensity, in candles.			Gas-consumption.			Increase in light-intensity[*], in per cents., in hot tubes, heated	
		In cold tubes.	In hot tubes, heated		In cold tubes.	In hot tubes, heated			
			near the orifice.	at some distance back.		near the orifice.	at some distance back.	near the orifice.	at some distance back.
X. XI.	Platinum tube.	0·4 2·0	0·9 3·4	0·6 2·8	19 48	19 46	19 45·5	125 78·9	50 55·5

These experiments prove that a much smaller increase in luminosity is brought about by heating the tube of the burner at some distance from its orifice than just at that orifice. These results confirm those of the former Tables, inasmuch as they show that the increase in luminosity occasioned by heating the burner-tube is more marked when the gas-consumption is small than when it is large. The fact that the luminosity of a flame produced by a large gas-consumption was increased in a proportionally greater degree than the luminosity of a small flame, when the tube was heated *in the middle*, is to be accounted for by remembering that in this experiment the rapid current of gas would give up proportionally less heat to the cold parts of the tube than the slowly moving stream would.

The maximum luminosity, then, is attained when the anterior part of the tube is heated: the cooling-down of the heated stream of gas is hereby prevented; and the cooling action of the cold burner upon the flame itself is at the same time removed.

The question now arises, Is the increased luminosity which is brought about by heating the tube to be traced merely to the increase in temperature of the gas, or to a change in the chemical composition of the gas itself? It may be supposed that heating the gas brings about the formation of new compounds which are very ready to suffer decomposition attended with separation of carbon in the flame. In order to determine whether this was so or not, coal-gas was passed through a heated platinum tube and then through a tube many times bent and surrounded with water maintained at 15°.

To this tube a steatite burner was attached. There was no observable alteration in the intensity of light from gas burning at the orifice of this apparatus.

It might be supposed that new compounds were actually produced in this experiment, but that the increased luminosity

[*] Calculated for constant consumption.

thus communicated to the gas was neutralized by the deposition of carbon within the platinum tube. No carbon, however, could be detected within the heated tube, even when the experiment had proceeded for a very considerable time. Special experiments also showed that when a stream of gas was very slowly passed for a considerable time through a tube of hard glass heated to redness, scarcely a trace of carbon was deposited. The deposition of large quantities of carbon within gas-retorts is to be attributed much more to the decomposition of condensed hydrocarbons than of marsh-gas and ethylene.

The gas used in the foregoing experiments, however, was almost perfectly free from condensed hydrocarbons and tarry matter; hence the non-deposition of carbon is only what might have been expected.

A small quantity of water containing tarry matter of a yellowish brown colour, collected in the cold tube through which the gas was passed after leaving the strongly heated platinum tube; inasmuch as this liquid was not formed unless the gas had been passed through the hot tube previously to being cooled, its formation must be traced to the decomposition brought about in the heated platinum tube among the constituents of the gas.

The quantity of this liquid, however, was too small to lead one to suppose that its production could exert any influence upon the luminosity of the flame. I passed pure hydrogen through the cold tube; the flame produced by igniting this hydrogen was non-luminous, even when the few drops of liquid in the tube were heated, showing the absence of liquid hydrocarbons such as benzene &c. I must therefore regard the results of this experiment as attributable to the composition of the special sample of coal-gas which was made use of.

The fact observed in previous experiments, that heating the burner caused an increase in the luminosity of the flame, must not, therefore, be referred to any decomposition brought about in the gas itself by the heat; for were it so the gas would have retained its increased illuminating power after being cooled to the ordinary temperature.

The direct addition of heat must therefore be regarded as the only, or at any rate the most important, cause of the increased luminosity.

Burners in which the temperature of the gas is raised before combustion are supposed to effect a very considerable increase in the luminosity of the gas-flame. By the use of such burners 60 per cent. more light may, it is said, be obtained than by using an ordinary fishtail burner.

These burners, however, heat the gas but slightly; moreover, from their construction, the heat used for increasing the temperature of the gas-stream is withdrawn from the flame itself, which is thereby cooled. The flame is caused to heat the tube of the burner, which then parts with its heat to the cold gas, being itself thereby reduced in temperature, and therefore brought into a condition in which it may exercise a cooling action upon the flame. Heat is withdrawn from the flame to be restored in diminished quantity, through the agency of the stream of gas. The logic of such burners is extraordinary.

I will not, on *à priori* grounds, deny that these burners may be useful under certain circumstances. In these cases, actions other than the heating of the gas are probably to be regarded as the true causes of the increased luminosity.

Burners whose effect depends upon their power of heating the gas before combustion, must be constructed so that the heat is not derived from the luminous flame itself, but either from the products of combustion or from an external source. Whether the quantity of gas or other materials consumed in the latter method would be repayed by the increased luminosity, is a purely economical question into which I cannot enter.

It might be supposed that the increase in luminosity of a flame, brought about by heating the burner-tube, was to be traced to the higher temperature to which the particles of separated carbon are raised. (I have already shown that the increase is not to be attributed to chemical decomposition within the burning gas.) But on looking more narrowly at the burning gas it will be seen that the flame-mantle becomes larger, that it stretches downwards towards the orifice of the burner, and that the lower portion, which was formerly blue, begins also to become luminous.

The conclusion which I draw from these phenomena is, that *the heated burner causes an earlier separation of carbon in the flame* than would otherwise take place. *Increase in the intensity of the light emitted by the glowing carbon particles, and earlier separation of these particles in the flame, are the actions brought about by heating the tube of the burner.* The total increase in luminosity registered by the photometer is the sum of these two actions.

The character of a flame is often conditioned, not only by the total luminosity, but also by the intensity of the light— that is, by the quantity of light emitted from a single constituent part of the luminous flame. I think that too little attention has hitherto been paid to this important fact. Although the photometer is unable to distinguish between a small but intense, and a larger but less-brilliant flame, never-

theless the difference between the two is apparent to the eye, which is able to look at the latter without shrinking, while it is unable to bear the intense light emitted by the former. The delicacy of the eye is therefore very different from that of the photometer. By covering the flame with a shade, arranged so as to permit the rays of light proceeding from a determined part of the flame only to pass outwards, it becomes possible to measure the luminosity of different parts of the flame by means of the photometer.

I may at a future time undertake a series of measurements by such a method as this; meanwhile I should like to put on record the fact that the light emitted by burning a certain petroleum, which is now much used, notwithstanding its high price, on account of the clearness and brilliancy of its flame, is considerably less, as measured by the photometer, than that obtained from ordinary commercial petroleum.

I propose to use the term *intensity of light* as applied to the quantity of light emitted by the individual constituents of the flame, and the term *light-effect* as applied to the total effect of the whole of the luminous particles, *i.e.* of the whole flame. Assuming that the light emitted by the luminous constituents is the same throughout, the "light-effect" would be equal to the product of the "intensity of light" into the volume of the flame-mantle. But inasmuch as this assumption is not proved good, it will not do in conducting photometric experiments to pay no attention to the part of the flame from which light is permitted to pass outwards through the opening of the shade. It is best in all cases to place the shade so that the greatest possible quantity of light may fall upon the diaphragm of the instrument; the results will then be comparable, so far as the most luminous part of the flame-mantle is concerned. It is just this part of the flame which most materially conditions the character of the whole.

The most important conclusions arrived at in the second part of this memoir are these:—A cold object brought into a luminous flame causes a suspension of the process of combustion in its immediate neighbourhood, and at the same time very materially diminishes the luminosity throughout a considerable space around itself.

Carbon-containing luminous materials may burn with or without separation of carbon (that is, with or without luminous flames), according as a certain temperature, differing for each material, is or is not maintained. Combustible material which has been diluted with indifferent gases requires to be maintained at a higher temperature, in order that it shall burn with a luminous flame, than when it is not so diluted.

The fact that soot is deposited on a cold object held in a luminous flame is no proof of decreased temperature within the flame, inasmuch as it has been shown that decreased temperature causes a diminution in the quantity, or even total suppression, of the separated carbon.

Soot is also deposited on heated surfaces, but is quickly burned on admission of air. Inasmuch as admission of air cannot be altogether prevented, less soot is accumulated on hot than on cold objects. The surface which collects the soot may be compared to a redoubt which stops the progress of the shots fired against it.

Burners constructed of iron or other material possessed of high conductivity for heat, cause a greater diminution in the luminosity of the lower part of the flame than burners of steatite. The difference between the luminosities of the two flames is rendered more apparent by artificially cooling the burner.

The top of the burner and the entering cold gas both exert a cooling action upon the lower part of the flame, and are the cause of the space noticeable between the burner and the flame-mantle. By heating the burner, and simultaneously the stream of gas, a more luminous flame is obtained without an increased consumption of gas, the increase in luminosity being greater the smaller is the consumption of gas.

The change which is hereby caused in the chemical composition of the gas is without appreciable effect upon the luminosity, inasmuch as no diminution in luminosity is occasioned by again cooling the stream of gas.

The heat communicated by increasing the temperature of the burner-tube acts in two ways: it increases the intensity of light of the flame-mantle, and simultaneously enlarges the flame itself. Carbon is also sooner separated in the flame, and separated at a higher temperature.

The eye is able to distinguish between these two actions; the photometer registers only the total effect.

It is very essential to distinguish between the " light-effect" of the whole flame, and the " intensity of light " (that is, the quantity of light emitted by the various constituent parts of the flame). For practical purposes (comparisons of different flames, &c.) it would be well to determine the total light-effect, and also the maximum quantity of light obtained by allowing the rays to pass through a small, accurately measured, opening in a shade placed between the flame and the diaphragm of the photometer. The numbers so obtained might be regarded as approximative values of the relative " intensities of light " of the most brilliant parts of the various flames.

XV. *On the Conversion of the Geyser-throats in Iceland into Volcanic Vents.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the 'American Journal of Science' for July of the present year, vol. xii. p. 25, in an interesting paper by Prof. C. G. Rockum, entitled " Notices of recent American Earthquakes," the following passage, referring to the eruption of Trölladyngja, in Iceland, which took place in January 1875, occurs :—

" Earthquakes preceded and accompanied an eruption of the volcano Trölladyngja, in the central part of Iceland.

" The volcanic disturbance appears to have begun by subterranean thunders during December 1874, extending through nearly two thirds of the island. Early in January 1875 earthquakes occurred in all directions ; and then an old extinct volcano near Vatrayskud opened, and for four weeks continued to emit ashes, lava, &c. When this eruption ceased, another extinct volcano near Myvatu, 100 miles further north, opened and continued in action for several weeks. Both of these eruptions occasioned great destruction of life and property. Early in March there seemed to be a general upheaval of the earth in the whole central portion of the island. The ashes from these and still later eruptions fell to the depth of several inches on the coast of Norway in the latter part of April. It is stated that ' the geysers have dried up since the eruption began, and, instead of water, emitted quantities of hot smoke and ashes.' "

If the latter part of this account be correct, it appears to me to lend support by actual fact to the views enunciated by me as to the " mechanism of Stromboli," read to the Royal Society in June 1874, and published in its ' Proceedings ' for that year, to which I refer your readers, as I must not occupy your space further than by stating here that I there attribute the rhythmical action of Stromboli to the concurrence in the same vent of a measured supply of heated water, or geyser, with the ejecta of a volcano of small energy. My views were objected to, with much vehemence of assertion, by the late Mr. Scrope, unaccompanied, however, so far as I can judge, by a single satisfactory argument.

Rightly considered, the fact of the sudden conversion of the geyser-throats in Iceland into volcanic vents seems to me to afford support to the probability that the mechanism assigned by me to Stromboli may truly represent the phenomena as

found in nature, and indicate that the term "preposterous," employed by the late Mr. Scrope in reference to my views, was misapplied. My main object in addressing you here is to call the attention of any competent observers who may visit Iceland during the summer of next year, so that a fuller and better account as regards the circumstances and conditions of this alleged conversion of geyser into volcano may be obtained.

<div style="text-align:center">

I am, Sir,

Your obedient servant,

ROBERT MALLET.

</div>

London, December 30, 1876.

XVI. *On the Specific Heats of Cerium, Lanthanum, and Didymium.* *By* Dr. W. F. HILLEBRAND [*].

THE following investigation has been made with the metals separated by electrolysis by Dr. Norton and myself[†], under the kind superintendence of Professor Bunsen, whose ice-calorimeter we have made use of.

The larger fragments of the metals (some of which weighed as much as 6 grms.) were most free from impurities, since the smaller globules, by falling through the mass of the fused chlorides, were apt to adhere to the sides of the porous cell, and to become contaminated with perceptible quantities of its material. Notwithstanding that this fact indicated that it was desirable to make use of the larger pieces of metal, I nevertheless hesitated to break up those beautiful preparations, rather preferring to employ the smaller and less pure specimens, and to determine the nature of the impurities after the specific heat had been ascertained and to make allowance for their effect. Since the metals under consideration are rapidly oxidized on exposure to air and decompose water, it was necessary to enclose them in a glass envelope. Accordingly it was first necessary to determine the amount of heat which 1 grm. of the glass employed gives out in cooling from $1°$ C. to $0°$ C. This, W_g[‡], is given by means of the equation

$$W_g = \frac{T}{G_g t}$$

from the following experiments:—

[*] From Pogg. *Ann. der Phys. u. Chemie*, vol. clviii. p. 71. Translated by Mr. John I. Watts, the Owens College, Manchester.

[†] Pogg. *Ann.* vol. clvi. p. 466.

[‡] Pogg. *Ann.* vol. cxli. p. 19. Phil. Mag. [IV.], vol. xli. p. 161.

		I.	II.
Weight of the glass	G_g	1·3287	1·8425
Initial temperature	t	99°·89	99°·804
Duration of the experiments	M_1-M_0	90′	80′
Scale-variation before experiment ...	$\dfrac{\tau_0}{m_0}$	0·16	0·13
Scale-variation after experiment ...	$\dfrac{\tau_1}{m_1}$	0·14	0·233
Corrected retraction.....................	Q_0-Q_1	371·7	522·2
Reduced and corrected retraction ...	T	385·2	536·72

$$W_g \text{ from Exp. I.} \quad . \quad . \quad . \quad = 2·902$$
$$W_g \text{ from Exp. II.} \quad . \quad . \quad . \quad = 2·919$$
$$W_g \text{ mean} \quad . \quad . \quad . \quad . \quad \overline{2·9105}$$

In order to calculate the specific heat S of the metals, the formula[*]

$$S = \frac{\dfrac{T}{t} - (W_g G_g + W_l G_l)}{W_w G_m} \quad . \quad . \quad . \quad . \quad (1)$$

is used, in which

G_g = weight of the glass envelope,
G_m = weight of metal employed,
G_l = weight of air enclosed with the metal in the glass envelope.

No special estimations of W_w were needed, since the ice-calorimeter employed was the same as that which Professor Bunsen had already made use of, and for which he had determined

$$W_w = 14·657.$$

W_l = the amount of heat expressed in scale-divisions which 1 grm. of atmospheric air gives out in cooling from 1° C. to 0° C. This amount of heat is given by means of the equation

$$W_l = W_u S_l,$$

in which S_l denotes the specific heat of the air at constant volume. By putting $S_l = 0·1668$, we have

$$W_l = 2·445.$$

In order to estimate G_l, the external volume V of the sealed envelope containing the metal was estimated in cubic centimetres by immersing it in a measuring cylinder containing water. Calling the weight of the glass envelope G_g,

its specific gravity .   .   .   .   . s ;

* Pogg. *Ann.* vol. cxli. p. 23.

further, the weight of the metal contained in
the glass envelope . . . . . G_m,
and its specific gravity . . . s_m,

and the temperature and pressure of the air in the glass envelope when sealed t and P, we have

$$G_l = \frac{\left(V - \dfrac{G_g}{s_g} - \dfrac{G_m}{s_m}\right) P}{773(1 + 0.00366\,t)0.760}. \qquad . \quad . \quad . \quad (2)$$

A. *Specific Heat of Didymium.*

The data for the calculation of G_l from formula (2) were

$$V = 1.17, \qquad\qquad s_g = 2.5,$$
$$G_g = 0.2445, \qquad\qquad s_m = 6.544,$$
$$G_m = 1.844. \qquad\qquad t = 40°,$$
$$P = 0.759;$$

and hence

$$G_l = 0.00080.$$

The experiments with the ice-calorimeter gave :—

	Weight of didymium, in grms.	Weight of glass envelope, in grms.	Weight of air in glass envelope.	Initial temperature.	Duration of the experiment.
	G_m.	G_g.	G_l.	t.	$M_1 - M_0$.
Exp. I. ...	1.8440	0.2445	0.00089	99°.90	50'
Exp. II. ...	1.8440	0.2445	0.00089	100°.10	70'

	Scale-variation before experiment.	Scale-variation after experiment.	Retraction of mercury, in scale-divisions.	Reduced retraction.
	$\dfrac{\tau_0}{m_0}$.	$\dfrac{\tau_1}{m_1}$.	$Q_0 - Q_1$.	T.
Exp. I. ...	−0.072	−0.035	199.5	196.8
Exp. II. ...	−0.117	−0.090	204.7	197.45

The specific heat of didymium is accordingly—

From Exp. I. 0.04649
From Exp. II. 0.04557
Mean . . . 0.04653

The atomic weights of the cerite metals hitherto adopted have in no case been directly determined, but have been estimated from the capacity of saturation of their salts. The estimation has been based upon the hypothesis, hitherto grounded on no well-established facts, that the amount of oxygen contained in them is one third of that present in the sulphuric acid needed to form the neutral salts. If, however, the composition of these salts were analogous to that of the uranium oxide, the percentage composition which is generally and unhesitatingly adopted for them would be completely erroneous. In order to test the validity of the hypothesis, it appeared to be of interest to fix definitely the amount of oxygen contained in the didymium oxide. For this purpose 0·9071 grm. of the same sample of didymium which had served for the determination of the specific heat of the metal was dissolved in nitric acid with all precautions to render loss by spirting impossible, and the solution was evaporated in a platinum crucible and carefully ignited. The weight of the oxide thus formed amounted to 1·0626 grm., and was unaltered by ignition in an atmosphere of hydrogen. When dissolved in hydrochloric acid and evaporated to dryness, it yielded, on re-solution in hydrochloric acid and water, 0·0071 grm. of silica.

The solution was then mixed with ammonia and the precipitate (A) filtered off; the filtrate, when evaporated and ignited, gave a residue which weighed 0·0028 grm., and proved to be didymium oxide. Precipitate A, digested for some time whilst moist with oxalic acid, gave an oxalate, from which 1·0640 grm. of didymium oxide was obtained by ignition, and a liquid, from which, after evaporation and ignition, 0·0044 grm. of alumina was obtained. On converting the oxide of didymium into chloride and testing it by the electric spark before the spectroscope, it showed weak lines of lanthanum, the amount of which, however, was inappreciable.

All these operations were performed in platinum vessels, and with reagents which were perfectly free from all impurity.

Tares of platinum were employed in all the weighings, in order to make the influence of the displaced air as small as possible.

The percentage composition of the oxide obtained from the metal is accordingly:—

I.

Didymium oxide	. . .	98·701
Silica		0·668
Ferric oxide		0·414
Alumina		0·179
Loss		0·038
		100·000

And the composition of the metal itself is—

<div style="text-align:center">II.</div>

Didymium	.	.	.	.	.	99·181
Silicon	.	.	.	.	.	0·368
Iron	.	.	.	.	.	0·339
Aluminium	.	.	.	.	.	0·112
						100·000

From this analysis it appears that the 0·9071 grm. of metal which was employed contained 0·8997 grm. of didymium, and gave, after removal of the impurity contained in the 1·0626 grm. of oxide, 1·0488 grm. of the pure oxide of didymium. The composition of the oxide of didymium, as found by direct analysis, is therefore—

<div style="text-align:center">III.</div>

Didymium	.	.	.	85·78 per cent.
Oxygen	.	.	.	14·22 ,,
				100·00

The percentage quantity of oxygen in didymium oxide deduced from P. T. Cleve's [*] analysis of the didymium sulphate, according to its saturation-capacity, is 14·04, agreeing very nearly with that directly found. The agreement would be still closer had it been possible to determine the amount of the lanthanum in the didymium.

Let $d =$ the amount of didymium contained in the unit weight of the metal under investigation,

$k =$ that of the silicon,

$e =$ that of the iron,

$a =$ that of the aluminium;

further, let the specific heat of these bodies be respectively s_d, s_k, s_e, and s_a, and let $S =$ the observed specific heat of the impure metal; then

$$ds_d + ks_k + es_e + as_a = S,$$

and hence

$$\frac{S - ks_k - es_e - as_a}{d} = s_d; \quad \ldots \quad (3)$$

by substituting the following values,

$$
\begin{aligned}
d &= 0·99181, & S &= 0·04653, \\
k &= 0·00368, & s_k &= 0·175, \\
e &= 0·00339, & s_e &= 0·114, \\
a &= 0·00112, & s_a &= 0·214,
\end{aligned}
$$

in this equation, we obtain for the specific heat of pure didymium the value 0·04563.

[*] *Abhandl. d. Schwedischen Akad.* vol. ii. No. 6.

Assuming that didymium oxide has the formula DiO, we obtain from analysis III. for the atomic weight of didymium the value 96·52[*], and for its atomic heat 4·40, which differs so considerably from the atomic heat of the other elements as to render the formula DiO completely invalid.   If, on the other hand, we adopt the formula $Di_2 O_3$, the weight of the didymium atom becomes one and a half times that of the above number, namely 144·78, and its atomic heat becomes 6·60, a number which agrees most satisfactorily with Dulong and Petit's law.

The oxide of didymium is accordingly, without doubt, a sesquioxide.

B. *Specific Heat of Lanthanum.*

The following data have served for the determination of G_l:—

		Exp. I.	Exp. II.
V	. . .	$= 0·60$	$1·00$
G_g	. . .	$= 0·1721$	$0·2154$
G_m	. . .	$= 0·8911$	$1·6828$
s_g	. . .	$= 2·5$	$2·5$
s_m	. . .	$= 6·049$	$4·049$
t	. . .	$= 40°$	$40°$
P	. . .	$= 0·756$	$0·758$

By substituting these elements in formula (2), we obtain

From Exp. I. . . $G_l = 0·00043$,
From Exp. II. . . $G_l = 0·00071$.

The following are the elements for the determination of the specific heat:—

	Weight of lanthanum, in grms.	Weight af glass envelope, in grms.	Weight of air in glass envelope.	Initial temperature.	Duration of the experiment.
	G_m.	G_g.	G_l.	t.	$M_1 - M_0$.
Exp. I. ...	0·8911	0·1721	0·00043	99°·76	49′.
Exp. II....	1·6828	0·2154	0·00071	99°·69	50′.

	Scale-variation before experiment.	Scale-variation after experiment.	Retraction of mercury in scale-divisions.	Reduced retraction.
	$\dfrac{\tau_0}{m_0}$.	$\dfrac{\tau_1}{m_1}$.	$Q_0 - Q_1$.	T.
Exp. I. ...	0·105	0·11	104·5	109·77
Exp. II....	0·058	0·08	174·6	178·05

[*] $O = 16$.

Hence we have for the specific heat of lanthanum:—

From Exp. I.	.	.	.	0·04582
From Exp. II.	.	.	.	0·04692
Mean		.	.	0·04637

The metallic lanthanum employed in these experiments was likewise not quite pure.   It was obtained from chloride of lanthanum after the larger globules of lanthanum had been separated, and in which the traces of didymium originally present had become concentrated, since lanthanum is more easily separated by the current than didymium.   0·8911 grm. of the same specimen which had been employed in the foregoing experiments, when oxidized with nitric acid, gave 1·0516 grm. of oxide of lanthanum, which, on solution in hydrochloric acid, evaporation, and re-solution, left 0·054 grm. of silica.

All the oxides were precipitated from the liquid by means of ammonia and filtered off; the filtrate, after evaporation and ignition, gave in addition a few milligrams of oxide of lanthanum.   By digesting the hydrates with oxalic acid an insoluble white residue of the oxalates of lanthanum and didymium was obtained, which, after ignition, together with the above-mentioned small residue, gave 1·0276 grm. of oxide of lanthanum containing didymium, and, on the other hand, a solution which, after evaporation and ignition, left a residue consisting of 0·0156 grm. of ferric oxide, 0·0026 grm. of alumina, and 0·0004 grm. of oxide of lanthanum.

The amount of oxide of didymium associated with the lanthanum oxide was determined by means of the photometric spectral method originally applied by Professor Bunsen.

For this purpose a solution was prepared which, in V_1 cubic centimetres, contained g_1 grm. of pure sulphate of didymium free from lanthanum.   The 1·0276 grm. of oxide of lanthanum which was to be tested for the amount of didymium which it contained, was then dissolved in sulphuric acid, and gradually mixed with so much water that both solutions, tested before the spectroscope in equally thick layers, showed the didymium bands of equal intensity.   When this point was attained the volume V of the liquid was read off.   From these data we obtain the amount of didymium g by means of the equation

$$\frac{V}{V_1} g_1 = g.$$

The experiment gave

$V_1 = 25\cdot7$ cubic centims.

$V = 23\cdot5$,,

$g_1 = 0\cdot0520.$

The 1·018 grm. of oxide of lanthanum contained accordingly 0·0476 of oxide of didymium.

The oxide obtained by the solution of the metal in nitric acid consisted therefore, in 100 parts, of

Oxide of lanthanum . .	= 93·23
Oxide of didymium . .	= 4·52
Oxide of iron	= 1·49
Silica	= 0·51
Alumina	= 0·25
	100·00

Since it follows, from the oxidation experiment above detailed, that 100 parts of oxide correspond to 84·737 parts of impure metallic lanthanum, which, after subtraction of the metallic impurities calculated from the analysis, contain 79·431 of pure lanthanum, corresponding to 93·229 pure oxide of lanthanum, we have for the percentage composition of the pure oxide as directly found :—

Lanthanum	85·20
Oxygen	14·80
	100·00

This composition is almost identical with that deduced from Cleve's [*] analysis of the lanthanum sulphate (made with great care for the special purpose of determining the atomic weight of lanthanum), on the assumption that the quantity of oxygen in the oxide is one third of that contained in the sulphuric acid necessary for saturation:—

Lanthanum	85·29
Oxygen	14·71
	100·00

The sample employed for the determination of the specific heat was found by analysis to have the following composition:—

Lanthanum	93·74
Didymium	4·59
Iron	1·23
Silicon	0·28
Aluminium	0·16
	100·00

By means of formula (3) the specific heat of pure lanthanum is found to be

$$0·04485.$$

Assuming, with Cleve, that the weight of the atom of lan-

* *Abhandlung. d. Schwedischen Akad.* vol. ii. No. 6.

thanum is 139, corresponding to the formula $La_2 O_3$, this number gives the atomic heat as $6\cdot23$; if we adopt the formula LaO, the atomic heat is $4\cdot15$, which does not agree with the law of Dulong and Petit.

The oxide of lanthanum must therefore likewise be regarded as a sesquioxide.

C. *Specific Heat of Cerium.*

The smaller globules of cerium were employed in this as in the former experiments, and for the same reason.

The value of the constant G_l is obtained from the following elements:—

$$V = 1\cdot17, \qquad s_g = 2\cdot5,$$
$$G_g = 0\cdot1601, \qquad s_m = 6\cdot728,$$
$$G_m = 2\cdot0935, \qquad t = 40°,$$
$$P = 0\cdot759.$$

It amounts to

$$G_l = 0\cdot00089.$$

The experiments with the ice-calorimeter gave:—

	Weight of cerium, in grms.	Weight of glass envelope.	Weight of air in glass envelope.	Initial temperature.	Duration of the experiment.
	$G_m.$	$G_g.$	$G_l.$	$t.$	$M_1 - M_0.$
Exp. I. ...	$2\cdot0935$	$0\cdot1601$	$0\cdot00089$	$99°\cdot70$	$60'$
Exp. II. ...	$2\cdot0935$	$0\cdot1601$	$0\cdot00089$	$94°\cdot93$	$60'$

	Scale-variation before experiment.	Scale-variation after experiment.	Retraction of mercury in scale-divisions.	Reduced retraction.
	$\dfrac{\tau_0}{m_0}.$	$\dfrac{\tau_1}{m_1}.$	$Q_0 - Q_1.$	$T.$
Exp. I. ...	$0\cdot060$	$0\cdot067$	$184\cdot6$	$187\cdot81$
Exp. II. ...	$-0\cdot040$	$-0\cdot026$	$188\cdot0$	$186\cdot42$

Hence we have for the specific heat of cerium:—

$$\begin{array}{llll}
\text{From Exp. I.} & . & . & . & 0\cdot04613 \\
\text{From Exp. II.} & . & . & . & 0\cdot04554 \\
\text{Mean} & . & . & . & 0\cdot04583 \\
\end{array}$$

In order to determine the composition of the metal under investigation, 0·7946 grm. of it was dissolved in nitric acid and the resulting nitrate ignited; the residual oxide of cerium weighed 0·9768 grm. By treatment with concentrated sulphuric acid and repeated evaporation with sulphurous acid 1·6303 anhydrous cerous sulphate was obtained, which, on solution, left a residue, consisting of 0·0031 grm. of silica and 0·0127 ferric oxide, which was found to be free from oxide of cerium and alumina.

The amount of didymium was determined by spectral analysis as above described; it was estimated at 0·0332 grm. The cerous sulphate, when converted into cerous chloride and tested in the electric spark before the spectroscope, exhibited only a few very weak lines of the lanthanum spectrum. The ceric oxide obtained by the oxidation of the metal contained in 100 parts:—

Oxide of cerium	=	94·98
Oxide of didymium . . .	=	3·40
Oxide of iron	=	1·30
Silica	=	0·32
		100·00

Since 0·9768 grm. of this impure ceric oxide was obtained from 0·7946 grm. of the impure cerium metal, 100 grms. of the above oxide must contain 81·347 grms. of the impure metal. By subtracting the amount of the metals contained in the impurities, we have, out of 94·98 grms. of pure ceric oxide, 77·362 grms. of pure cerium metal.

The composition of the ignited oxide obtained after treatment of the pure metal with nitric acid as directly found is therefore

Cerium	81·45
Oxygen	18·55
	100·00

According to very careful experiments upon chemically pure cerous sulphate, which were performed in Professor Bunsen's laboratory some time ago by C. Wolf, this salt contains

Cerous oxide	=	57·294
Sulphuric acid	=	42·706
		100·000

On the purely hypothetical assumption that in this salt the quantity of oxygen is one third of that contained in the sulphuric acid needed for combination, it follows that the composition of the highest oxide of cerium is

$$\begin{array}{lr}
\text{Cerium} & 81 \cdot 06 \\
\text{Oxygen} & 18 \cdot 94 \\
\hline
& 100 \cdot 00
\end{array}$$

closely agreeing with that above determined.

The foregoing analysis gives for the composition of the specimen employed in the determination of the specific heat:—

$$\begin{array}{lr}
\text{Cerium} & 95 \cdot 10 \\
\text{Didymium} & 3 \cdot 60 \\
\text{Iron} & 1 \cdot 12 \\
\text{Silicon} & 0 \cdot 18 \\
\hline
& 100 \cdot 00
\end{array}$$

from which we find, by formula (3), the following value for the specific heat of pure cerium,

$$0 \cdot 04479.$$

If we assume the formula of the lowest oxide of cerium to be CeO, then the atomic weight of the metal becomes 92, and its atomic heat 4·12. If, on the other hand, we regard cerous oxide as a sesquioxide, we find the atomic weight 138, and the atomic heat 6·18, which agrees in a most satisfactory manner with the atomic heat of the other metals. We must therefore adopt the following formulæ as representing the composition of the oxides of cerium:—

$$Ce_2 O_3,$$
$$Ce\ O_2.$$

XVII. *On the Magnetization of Steel by Currents.* By E. BOUTY, *Docteur ès Sciences* [*].

Introduction.

A MAGNET may be regarded as a combination of a great number of elementary magnets, differing from one another in the directions of their axes and the amounts of their magnetic moments. The actual distribution of these *magnetic elements* (of which we owe the notion to the experiment of the broken magnet) remains inaccessible to experiment, at least as long as the integrity of the magnet under investigation is preserved; but in most cases some restrictions are imposed beforehand on the problem which simplify it much.

In the first place, we demonstrate that for true magnetiza-

tion * a fictitious superficial distribution of polar (austral and boreal) magnetism may be substituted, which replaces it in respect of all the actions exerted by the magnet exteriorly to its mass.   This is the distribution usually studied since Coulomb; and the knowledge of it is sufficient as long as the magnet is not disunited.

In the second place, when the practical case of a bar magnetized regularly is considered, it is observed that the magnetic distribution of the bar is limited to two regions, equal in quantity and of opposite signs, occupying the two extremities.   We may suppose each of these masses condensed in its centre of gravity; and the bar will then be replaced by two magnetic poles of which the masses are m and $-m$, and the distance of the one from the other a quantity λ†.

Lastly, in regard to the actions at infinite distance one magnet differs from another by one element only, which is designated under the name of *magnetic moment*, and of which the rational measure, in the case of a regular bar, is the product $m\lambda$ of the *quantity of magnetism* of each pole by their distance.   Such is the term of this analysis.

The methods to which recourse is had for the experimental study of magnets are of two kinds: the one sort, utilizing action at contact or at minute distance, are employed for determining the magnetic distribution: their employment is tedious and laborious, and their application subject to special theoretical difficulties.   The others, founded upon action at very great distances, furnish in a manner as simple as it is accurate the measurement of the magnetic moment.

I purpose in this investigation to apply these latter methods to the study of the distribution.   Biot has already set the example of this kind of research, by combining in a mathematical formula (subsequently connected by Green with the

* See, in the *Journal de Physique*, tome ii. p. 297, my article " Sur les distributions fictives d'électricité et de magnétisme," &c.

† Physically we may, with M. Jamin, consider a bar as a bundle of indefinitely thin magnets having their poles at their extremities. This bundle, embraced by the mean section of the bar as by a ring, expands on the two sides its oppositely named poles, which form the superficial distribution.   This synthesis amounts to replacing the unknown distribution of the magnetic elements by the equivalent solenoidal distribution (see Thomson's memoirs), and is perfectly legitimate from the mathematical point of view.   It has the advantage of addressing the imagination, and of giving a precise physical meaning to all the quantities met with in the analytical study of magnets.   Thus the power m of the pole of a bar measures the number of files (rows) of magnetic elements comprised in it; and the distance λ is the mean length of the files, reckoned in the direction of the axis: it may be called the *reduced* or *magnetic length* of the bar.

theory of the coercive force*) the laws of the distribution in a series of saturated cylindrical bars with the magnetic moments of the bars. The portion of my memoir which relates to large bars is only a wider development of Biot's method; that relative to long and thin needles is more original, and its purpose is to determine independently of any hypothesis the two factors m and λ of the magnetic moment.

Given that we possess a series of magnetized needles in which the quantity of magnetism m is the same, as well as the distance $\dfrac{d}{2}$ from one pole to the nearer extremity. This will be the case when the needles are obtained, by breaking, from the middle of one and the same strongly hardened needle †; and we shall see in the sequel that it is a very general one. Then let y be the magnetic moment of a needle of which the length is x; we have

$$y = m\,(x-d)\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and, theoretically, two measurements, made upon needles of different lengths, will be sufficient for determining the two quantities m and d.

This, I believe, is the first time that determinations have been published relative to the situation of the poles in needles magnetized by currents. As regards the quantities of magnetism, their investigation has been the object of the researches of a great number of physicists, of which I have elsewhere ‡ cited the principal and most recent. The application of the methods employed requires great delicacy, and supposes the possession of considerable homogeneous masses of the metal under investigation—a condition very difficult to fulfil. Moreover these masses must take the form of ellipsoids§ or of rings‖ for the magnetization they receive to be the same in all their points; then the quotient of their magnetic moment by their volume gives the quantity of magnetism μ referred to the unit of volume.

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* *Annales de l'École Normale Supérieure*, 2ᵉ série, t. iii. p. 34.
† Ibid. pp. 36, 43.   ‡ Ibid. p. 10.
§ Poisson has demonstrated *(Mémoires de l'Académie des Sciences,* t. vii.) that, in an ellipsoid submitted to the action of a constant magnetic force acting in the direction of its major axis, the magnetization is identical in all the points of the mass, and equal to the value which it has at the centre of an infinitely long cylindrical needle submitted to the same force. Quintus Icilius employed ellipsoids.
‖ Stoletow and Rowland made use of rings. The magnetization is the same in all the points by reason of symmetry; but, a closed solenoid being without action upon any exterior point, they were obliged, in order to accomplish their measurements, to produce induced currents accompanying the magnetization or demagnetization of the metallic ring.

I have effected only relative measurements of μ; but the physicists who have studied the question have found its values so variable from one steel or soft iron to another, and even for one and the same sample, in physical circumstances apparently so closely similar, that there is but little interest in realizing absolute measurements as long as the laws which govern this variability are not entirely known to us.

The greater part of these researches were effected at the Lycée of Rheims; they were finished in the laboratory of M. Jamin. Permit me here to return thanks to that able master for the kindness with which he has welcomed me and for the counsels which he has not ceased to lavish upon me.

I. *The Permanent Magnetization of thin Needles tempered hard.*

The quantity of magnetism acquired temporarily or permanently by a steel needle submitted to the action of a current depends on the intensity of the *magnetizing force* employed. It is important first of all to define the latter with precision.

In order to magnetize it, the needle is placed in the axis of a helix very long relatively to the needle, and sufficiently wide. Under these conditions the action exerted by the current is the same at all the points of the needle, and proportional to the intensity of the current and to a coefficient which depends on the number and width of the turns; but if we limit ourselves to employing always the same helix and to relative measurements, we can take the measure of the intensity of the current for that of the action exerted upon the needle—that is to say, of the magnetizing force.

The relative measurement of the intensity of the currents was performed by means of a sort of tangent-compass of very simple construction. A small helix A B (fig. 1), excited by

Fig. 1.

the current is adjusted so that its axis, sensibly perpendicular to the magnetic meridian, passes through the centre o of a small magnetized needle $a\,b$, furnished with a mirror and suspended by a cocoon-fibre. The tangent of the deflection produced is proportional to the current-intensity [*]. Great

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[*] Suppose the needle $a\,b$ and the helix A B indefinitely small in comparison with the distance $Ao = r$. The helix can be replaced by a magnet of which the moment M is approximately equal to the product Si of the sum of the surfaces embraced by each turn separately multiplied by the

care must be taken, in order to eliminate all action extraneous to the helix, to recurve the two electrodes which convey the current to it the one upon the other, and very close, so that their action upon an exterior point shall be very sensibly *nil*. A commutator permits the reversal of the current in the helix, and thus the elimination, by a second measurement of the deflection in the opposite direction, of the error resulting from the imperfection of the adjustment.

With respect to the measurements of magnetic moments, they were usually effected by the method which I have previously described for very small magnetized needles [*].

I have investigated, in the first place, the permanent magnetism of thin needles tempered very hard. These needles have a length at least equal to, and generally above, fifty times their diameter. It is easy to verify that the moments y acquired permanently by these needles submitted to the same magnetizing force are represented by the formula

$$y = m\,(x - d)\,;$$

and consequently the method indicated in the Introduction is applicable to such a series of needles [†]. It would therefore suffice to measure the corresponding values of x and y for a great number of needles of the same diameter and different lengths, and to make all the observations cooperate for the determination of m and d.

But here the method presents an inconvenience: it is in fact very difficult to communicate to a great number of needles a truly identical degree of hardening; and if this condition is not realized, one of the two determinations, that

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intensity of the current. If now a be the deflection of the needle $a\,b$, μ its magnetic moment, we have, after Gauss,

$$\tan a = \frac{2\mathrm{M}\mu}{\mathrm{T}r^3} = \frac{2\mathrm{S}\mu}{\mathrm{T}r^3}i.$$

If the lengths of $a\,b$ and $\mathrm{A\,B}$ are no longer negligible in comparison with r, the coefficient of i is much more complicated, but $\tan a$ remains proportional to i.

[*] *Annales de l'École Normale,* 2e série, t. iii. p. 12. This method, absolutely faultless in the case of very small needles, sometimes becomes a little faulty when one wishes to measure the moment of very long ones very feebly magnetized. This is in consequence of its not being permissible in the latter case to suppose no reciprocal influence between the directing bar and the needle. But this cause of error, when present, is betrayed by the non-accordance of measures 1 and 3; and the corresponding determinations are rejected.

[†] Equation (1) represents a right line, on the condition that x and y be regarded as current coordinates. The simplest way of proving the applicability of this formula consists in verifying that the characteristic points of the various needles really fall in a straight line.

of d, becomes almost illusory. Relying, however, upon a result of experiment which I have already indicated elsewhere [*], we may determine m and d by means of only one needle. When a regular magnetized needle, tempered hard, is broken, the different fragments taken from its middle portion, and of sufficient length, have magnetic moments represented by

$$y' = m\,(x' - \delta). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The quantity δ is independent of the intensity of the magnetization; and it is almost evident *à priori* that the quantity m is the same as in the mother needle. Besides, we demonstrate it experimentally; remarking that equations (1) and (2), in which x and y are considered as current coordinates, represent two straight lines, we have only to trace these by a sufficient number of observations, and prove that they are parallel—that is, that the two quantities m are equal.

This being admitted, let us determine the length and the magnetic moment, x and y, of a needle, then reduce it by ablation of the two ends to a suitable length, and determine again the magnetic moment y' and the corresponding length x'. The distance $\dfrac{\delta}{2}$ of the pole of a rupture-needle from its extremity is known beforehand, and is equal, for example, to 2·75 millims. for a needle of 0·55 millim. diameter. Equations (1) and (2) therefore completely determine m and d by means of one needle only. Let us add that we can further shorten the primitive fragment by successive breakings, and obtain as many points as we please, in order to determine better the straight line (2)—that is, the value of m. The quantity of magnetism will thus be ascertained to a very great nicety [†].

[*] *Annales de l'École Normale,* 2ᵉ série, t. iii. p. 43.

[†] Let B C and A D (fig. 2) be the straight lines represented by equa-

Fig. 2.
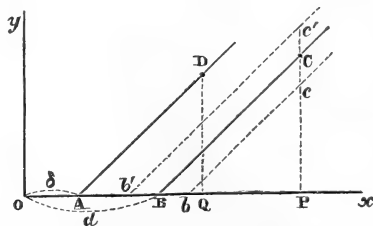
tions (1) and (2). The problem of the determination of d comes geometrically to the following—through a given point C to draw C B

The determination of d is, to be sure, less precise, since it rests upon a single observation, that which refers to the mother needle, and, besides, d is always a rather small quantity; still, by multiplying observations and taking the means, satisfactory results can be arrived at.

Quantities of Magnetism.—The following results refer to needles of 0·553 millim. diameter; the numbers of both columns are expressed in arbitrary units :—

Intensity of the current.	Quantity of magnetism.
3	hardly sensible.
5	0·12
7	0·51
9	1·09
12	2·11
13·8	2·89
15	3·35
18	5·65
23	11·96
28	17·90
36	23·00
40	24·00
50	25·90
∞	28·90

In order to represent better the course of the function m, a curve can be constructed by taking the intensities for abscissæ, and the corresponding values of m for ordinates. At first concave towards the positive ordinates, the curve then presents an inflection-point corresponding to the abscissa 22 nearly, and approaches asymptotically a parallel to the axis of the abscissæ.

These characters are identical with those of the curves which, according to Rowland and Stoletow, represent the magnetizing function of iron or steel [*]. The general features are everywhere the same; and the resemblance is especially striking when, opposite the preceding curve, we draw that found by Rowland for Bessemer steel [†].

It is nevertheless expedient to examine if there is for tempered steel a true magnetizing-function—that is, if the quo-

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parallel to a given straight line A D. The line A D is perfectly known; as to the point C, its position on the ordinate C P admits an error, in excess or defect, C c, whence results for D an error, negative or positive, equal to B b.

[*] This is the quantity of magnetism μ referred to unit volume and regarded as a function of the intensity of the magnetizing force.

[†] Here is the Table which has served for tracing this curve; it is deduced from the original memoir published, in August 1873, in the 'Phi-

tient of the quantity *m* by the square of the diameter preserves, for one and the same value of the current-intensity, a constant value. The difficulty of ascertaining this resides in the inequality of the hardening communicated to needles of different diameters placed in the same external conditions—for instance, on immersing them in water when they exhibit the same bright red *.

On the other hand, if we compare the phenomena of the steeping of steel with those of the steeping of iron, we are led to attribute to the steeped steel a certain degree of heterogeneity from the surface to the centre; and if it really existed, one could no longer talk of a magnetizing function of steeped steel. Therefore, upon the advice of M. Jamin, I tried dissolving in an acid † the superficial layer of steeped needles, so as to reduce them to less diameters, and afterwards magnetizing the needles thus obtained. In this way I proved, not without some surprise, the existence of a perfectly determinate magnetizing-function. The steeped steel of my thin needles is, then, very sensibly homogeneous ‡ ; and the experiment in question permits us to state precisely what should be understood by the rather vague expression *identical temper* when applied to needles of different diameters. The following Table is intended to prove the existence, in the present case, of a magnetizing-function; it relates to the magnetism acquired permanently by needles thinned with acid from an initial diameter equal to 1·178 millim.

losophical Magazine;' the numbers of both columns are expressed in absolute units:—

Intensities.	Quantities.	Intensities.	Quantities.
0·1356	18	2·756	26880
0·2793	80	3·219	34200
0·5287	255	3·551	40320
0·9398	727	4·469	52940
1·421	2526	5·698	61920
1·880	5108	11·44	91530
1·947	6482	20·69	96940
2·300	13510	38·99	100770

* In this case it is the thickest needles which are the most hardened; they approach more slowly than thin needles the maximum of magnetism of which they are susceptible. Nevertheless, as I have demonstrated (*Ann. de l'École Normale*, 2ᵉ sér. t. iii. p. 40), this maximum verifies the law of the diameters.

† Boiling chlorhydric acid, or aqua regia.

‡ The outermost layer, to a depth of 0·05 millim. at the most, possessed perhaps different properties; but to decide this question requires new experiments.

	Initial diameter **1·178** millim.				
Current.	Diameter.				Mean.
	1^{mm}·088.	0^{mm}·948.	0^{mm}·854.	0^{mm}·762.	
19·55	0·1800	0·1722	0·1202	......	0·1631
27·91	0·3709	0·3764	0·3350	0·3783	0·3663
36·25	0·6298	0·6424	0·6078	0·6821	0·6424
45·50	0·8697	0·8863	0·9078	0·8712	0·8881

To avoid the errors which might result from an inaccurate valuation of the diameter*, this Table contains, opposite to each value of the current-intensity, the fraction of the extreme quantity of magnetism attained by each group of needles. These numbers should be equal in one horizontal line, if there really exists a magnetizing-function.

Here are, besides, the values of the quotients $\mu = \dfrac{M}{D^2}$:—

D.	$\dfrac{M}{D^2}.$
millim.	millim.
1·088	0·1346
0·948	0·1291
0·854	0·1095
0·762	0·1125
Mean	0·1214

The slight decrease in μ is to be attributed to the valuation of the last two diameters being a little too high, as explained in the note.

The results relative to the magnetizing-function of tempered steel have been confirmed by the study of bars of 7–10 millims. initial diameter.

Distance of the Poles from the Extremities.—The following Table gives the double distance of one pole from the nearest extremity for needles of 0·553 millim. diameter:—

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* The corroded needles present microscopic holes, minute solutions of continuity; the estimation of their diameter, made with the spherometer, is therefore a little too high.

| Current. | *d.* | | Difference. |
	Observed.	Calculated.	
	millim.	millim.	
18·09	12·37	12·37	0·00
24·99	11·86	11·32	+0·54
30·31	10·12	10·52	−0·40
39·32	8·86	9·16	−0·30
47·04	7·32	7·99	−0·67
55·07	7·19	6·78	+0·41
∞	5·50		

It is seen that the poles approach the extremities in proportion as the intensity of the magnetizing current is augmented. The numbers in the third column were calculated by the formula $d = 15 \cdot 1 \left(1 - \dfrac{x}{100} \right)$; and we see that they very well represent the facts of experiment, considering the errors of which the measurement of d is susceptible; in the vicinity of saturation, however, the decrease of d is slower than would be indicated by the formula, and the values of d asymptotically approach 5·5 millims., which is characteristic for saturated needles of this diameter.

We know that, when the diameter of the needles changes, the limit of d is proportional to the diameter[*]; therefore the general formula for the distance of the poles is

$$d = 2a\mathrm{D}(1 - px), \quad \ldots \quad \ldots \quad (3)$$

where a and p are constants. The constant a depends on the degree of tempering, as will be seen in the sequel. Here the value is $a = 13 \cdot 65$; it represents in millimetres the limit towards which the distance $\dfrac{d}{2}$ for a needle of 1 millim. diameter tends when the force which produces the magnetization tends towards zero. The value of the coefficient p depends on the unit by means of which the magnetizing force is expressed. The limit of d for $x = \infty$ is

$$\delta = 2a\mathrm{D}; \quad \ldots \quad \ldots \quad \ldots \quad (4)$$

and the absolute value of α is 4·973 millims.[†]

In brief, and under the restriction that all the needles em-

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[*] See *Annales de l'École Normale*, 2$^\text{e}$ sér. t. iii. p. 40.

[†] Experiments made on thicker bars by the methods which will be indicated further on have furnished results accordant with these. Thus bars 10 millims. in diameter, steeped at a red heat, gave for a the value 13·75 millims., nearly identical with the preceding; and yet the value of α remained equal to 8—doubtless because saturation could not be attained, notwithstanding the employment of the current of twenty-five good Bunsen elements.

ployed were obtained by erosion from the same mother needles, there is a magnetizing-function $\phi(x)$; and the magnetic moment of a needle of length l and diameter D is represented by the formula

$$y = \frac{\pi D^2}{4} \phi(x)[l - 2\alpha D(1 - px)], \quad . \quad . \quad . \quad (5)$$

up to values of y very near to the limit Y corresponding to saturation. The limit itself is given by the formula

$$Y = \frac{\pi D^2}{4} L(l - 2\alpha D), \quad . \quad . \quad . \quad . \quad (6)$$

L being the limiting value of the magnetizing-function for $x = \infty$.

Repetition of passing the Needle to the Helix.—I have already pointed out [*] that, when the passage of the needle into the magnetizing helix is repeated, its magnetic moment increases so as to be represented by the formula

$$y = A - \frac{B}{n}, \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where A and B are constants depending on the intensity of the magnetizing force, and n indicates the number of the passages. It was of importance to seek to ascertain if the augmentation of the magnetic moment in question proceeded from a mere change in the distribution of the magnetism (that is to say, a displacement of the poles towards the extremities), or if there was a real augmentation of the quantity of permanent magnetism preserved by the needle.

The method above employed permitted me to solve this question in a satisfactory manner. I found that in these circumstances the quantity of magnetism and the situation of the poles change simultaneously; but the alteration of the latter was scarcely perceptible. Formula (7) applies very well to the quantities of magnetism considered separately.

If, starting from zero, the intensity of the magnetizing force x be progressively increased, the ratio $\frac{A}{A - B}$ of the quantity of magnetism for ∞ passages, to the quantity corresponding to the first passage, diminishes and tends toward unity [†]; but

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[*] *Annales de l'École Normale,* 2e sér. t. iii. p. 22.

[†] In a recent paper (Pogg. *Ann.* Ergänzungsband vii.) M. Fromme expresses doubts concerning this result. According to him, the number of *impulsions* required by a needle in order to acquire all the magnetism which can be communicated to it by a given force increases with the intensity of that force, and saturation can only be obtained by an infinity of impulsions.

This, to say the least, strange result was obtained by employing very

what is remarkable is, that if we seek, by means of the curve which represents the magnetizing function for the needles considered, to discover what the intensity x' of the magnetic force would have to be to produce the quantity of magnetism A after a single passage, we find for the ratio $\dfrac{x'}{x}$ a sensibly constant value.

The following Table refers to needles of 0·553 millim. diameter:—

Current x.	A — B.	A.	$\dfrac{A}{A-B}$.	$\dfrac{x'}{x}$.	
10	1·00	1·28	1·280	1·080	
14	2·80	3·49	1·246	1·075	
18	5·80	6·76	1·165	1·058	
22	10·60	12·19	1·150	1·055	
26	15·70	17·50	1·115	1·061	1·063
30	19·70	21·37	1·085	1·077	
34	22·20	23·49	1·058	1·112	
38	23·60	24·45	1·036	1·102	
42	24·50	25·11	1·025	1·073	
			Mean	1·077	

The values of the ratio $\dfrac{x'}{x}$ united by the brace are those which are considered the best-determined, because they refer to the region of *rapid magnetization*, of which we shall subsequently speak. The corresponding mean differs but little from the general mean, considering the multiple causes of error which must accumulate in the calculation of $\dfrac{x'}{x}$.

It has moreover been observed that the value of this ratio varies very little with the diameter of the needles, which we suppose always below 1 millim.; its mean value appears to lie between 1·060 and 1·065. It was natural to suppose that the displacement of the poles corresponds exactly to the increment of the quantity of magnetism—that is to say, that the final magnetization is in all points identical with that which

inconsiderable magnetizing forces acting on ellipsoids. It appears to me that under these conditions a multitude of perturbing causes must act. As, however, I have only employed the action of intense currents, I abstain from giving an opinion on this matter. But if M. Fromme will take the trouble to magnetize a simple hard-tempered needle in a powerful helix excited by ten Bunsen elements, he will have no difficulty in convincing himself that in this case saturation is attained at the first stroke, as is known by all physicists

would be directly produced by the magnetic force x'. Experiment has verified this anticipation in a satisfactory manner when we consider the smallness of the displacements to be measured. The following are the numbers belonging to needles 0·553 millim. in diameter:—

Current x.	d (1st passage).	d (5th passage).	Difference.	
			Observed.	Calculated.
33·29	millim. 10·073	millim. 9·730	+0·343	+0·309
47·88	7·869	7·570	+0·299	+0·444

II. *Temporary Magnetization of thin Needles tempered hard.*

In order to study the temporary magnetization of the needles, the magnetizing helix is placed in a groove perpendicular to the magnetic meridian, and of which the prolongation meets the centre of the galvanometer-needle which served us in the preceding section. The deflection produced by the helix alone is proportional to the intensity of the current, as already explained[*]. When a needle is introduced into the helix, the deflection is increased by a quantity which may serve as a measure for the temporary magnetic moment of the needle, but only under certain conditions, which we will state precisely.

The deflection produced by the needle whose temporary moment is M is given by the formula of Gauss,

$$\tan \alpha = \frac{2M\mu}{Tr^3}, \quad \ldots \ldots \quad (1)$$

provided that the length of the needle may be neglected in comparison with the distance r of its centre from the galvanometer-needle. If it be otherwise, Gauss's formula must be completed by multiplying the second member by a factor whose first term is unity, and the succeeding terms of the order of the powers 2, 4, 6, ... of $\frac{l}{r}$. The values of $\tan \alpha$ remain proportional to M for one and the same needle, or needles of the same length, but cease to be so for needles of different lengths whose centres coincide.

[*] To render the present experiments comparable with those of the preceding section, the ratio of the deflections α and α' must be determined which are impressed upon the needle by one and the same current traversing the galvanometric helix of the preceding section and the large magnetizing helix, and the new measures of the intensity must be multiplied by the ratio $\frac{\alpha'}{\alpha}$.

Hence arises the necessity of placing the helix at a great distance from the galvanometer-needle, and consequently the obligation to be contented with moderate or small deflections in order to estimate the moments.   I have recently obviated this inconvenience by rendering the galvanometer-needle nearly astatic with the aid of a compensating bar placed above the needle in an invariable situation.

There are strong reasons for admitting, *à priori*, that the distance of the temporary poles of the needles from their extremities is independent of the intensity of the current.   In fact, during temporary magnetization the needles are submitted to a magnetic force which has the same value in all points of them—that is to say, to a force analogous to the coercive force introduced by Green into his calculations upon saturated needles*; the distribution of the magnetism and the situation of the poles will be the same in both cases.   The same observation was applicable to rupture-needles†, for which we have directly verified that the poles occupy an invariable situation‡.

We will provisionally accept this conclusion, which I have verified most completely in the case of bars.   Thus the determination of the temporary moments is equivalent to that of the quantities of magnetism when the distance $\dfrac{d'}{2}$ from one temporary pole to the nearer extremity is known from experiments on saturated needles.   It has appeared convenient, in order to eliminate as far as possible every cause of error, to compare the deflection produced by the temporarily magnetized needle with that which it produces in the same situation by its residual magnetism after the suppression of the current.

Let ρ be the ratio of these deflections, m' and m the quantities of temporary and permanent magnetism, δ the double distance from one temporary pole to the nearer extremity, d the same quantity for the permanent magnetization (determined by formula 3 of the preceding section), and r the ratio $\dfrac{m'}{m}$; we have as a very close approximation,

$$\rho = \frac{m'(l-\delta')}{m(l-d)} = r\frac{l-\delta}{l-d}. \quad . \quad . \quad . \quad . \quad (2)$$

---

* I have verified that the formula of Green is conformable with the results of experiment, as regards saturated needles: see *Annales de l'École Normale*, 2ᵉ série, t. iii. p. 37.

† At the moment when a needle is detached from the centre of a much longer needle, it is found to be subject to the same magnetic force in every one of its points.

‡ *Annales de l'École Normale*, 2ᵉ série, t. iii. p. 43.

The following Table shows the values found for r with needles of 0·553 millim. diameter:—

Current.	ρ.	r.		Difference.	Weight.
		Observed.	Calculated.		
8·45	8·573	7·290	6·399	+0·891	2
10·56	4·255	3·590	3·932	−0·342	1
11·79	3·819	3·058	3·237	−0·179	4
12·81	3·293	2·765	2·848	−0·083	1
14·39	3·089	2·641	2·438	+0·203	2
15·25	3·000	2·271	2·279	−0·008	1
17·27	2·409	2·086	2·011	+0·077	1
18·75	2·073	1·727	1·877	−0·150	2
20·28	2·167	1·834	1·773	+0·061	6
23·56	1·919	1·622	1·623	−0·001	5
26·50	1·624	1·399	1·539	−0·150	6
31·32	1·581	1·374	1·442	−0·078	7
36·24	1·506	1·390	1·400	−0·010	2
41·79	1·462	1·331	1·368	−0·037	3
68·68	1·343	1·340	1·297	+0·043	7

The numbers of the last column indicate the number of needles employed for the determination of the corresponding value of ρ. In order to avoid any systematic error proceeding from the calculation of r, for different intensities of the current, needles of different lengths were employed, the centre of which was always placed at a fixed distance from the galvanometer-needle[*].

The numbers in the fourth column were calculated by the formula

$$r = 1 \cdot 2619 + \frac{a}{(x-3)^2} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

with $\log a = 2 \cdot 18355$; x represents the intensity of the current. The same formula suits very well to express the ratio r for thicker needles, 0·687 and 0·957 millim. in diameter for instance, on condition of making $a = \alpha D$, α being a constant[†]. A similar formula also suits for the Bessemer steel investigated by Rowland, except for the highest intensities; the ratio r then presents a notable augmentation, observed also in some of my experiments, and to which we shall revert by-and-by.

We have already seen that, properly speaking, there is no function of permanent magnetism for tempered needles of dif-

[*] For this the needle under experiment is kept at the bottom of a tube closed at one end, which fits with friction into the magnetizing helix. This tube is divided into half-millimetres; and its position is regulated by means of a fixed mark traced upon the tube which carries the helix.

[†] This empirical formula succeeds very well for needles of from 0·45 to 1·50 millim. steeped at a dull red-heat, but is far from being general.

ferent diameters ; nor is there for those needles a real function
of temporary magnetism ; and we have just seen that, between
certain limits of thickness, the ratio of the two quantities of
magnetism depends on the diameter—a result perfectly incom-
patible with the existence of these two functions.   Yet if,
tempering a series of needles of the same diameter, we after-
wards reduce them to different diameters by the action of an
acid, we know already that there is a well-determined function
of permanent magnetism ; and there is also a function of tem-
porary magnetism: the ratio r becomes independent of the
diameter ; it retains for all the needles the value corresponding
to the diameter of the mother needles according to formula (3).
The following Table refers to needles of initial diameter equal
to 1·178 millim.:—

Current.	r.		Difference.
	Mean value.	Calculation.	
19·55	2·323	2·449	−0·126
27·91	1·862	1·786	+0·076
36·25	1·609	1·556	+0·053
45·50	1·498	1·442	+0·056

The second column gives the mean of the values of r ob-
tained with superficially corroded needles reduced to 1·088,
0·348, 0·854, and 0·762 millim.

We can also assure ourselves directly that the function of
temporary magnetism is constant, as we have that the function
of permanent magnetism is so.   The curve T (fig. 3) was ob-

Fig. 3.
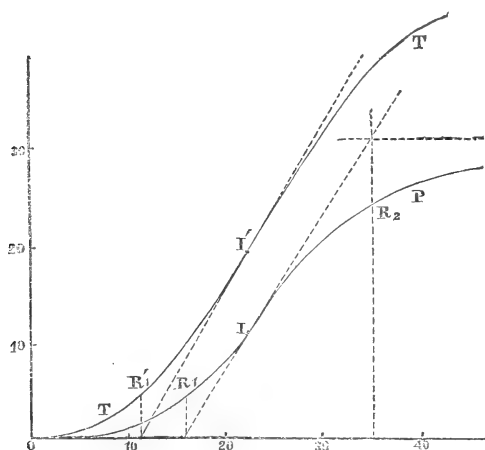
tained by means of the needles of 0·553 millim. diameter. It is seen that it presents the same general characters as the curve P, which relates to the permanent magnetism. The portion of the curve, R_1, R_2, comprised between the foot of the tangent to the point of inflection and the point where this tangent cuts the horizontal asymptote of the curve is the region of *rapid magnetization*. The extent of this region, and the coordinates of the two curves corresponding to the extreme points, very well characterize, in default of the complete curves, the magnetic qualities of the steel employed.

[To be continued.]

XVIII. *Note on the Optical Characters of Ludlamite.*
By N. S. MASKELYNE, *M.A., F.R.S.**

IN my contribution to Mr. Field's paper on the above mineral I gave a very brief and inexact notice of its optical characters, not having the time or the material to work them out. The preliminary experiments had been made upon fragments of rough crystal on which only the cleavage-plane could be identified, and the form of which was misleading as to the position of the face 1 0 0. I have since had the opportunity of working upon another crystal, which, though very full of divisional planes, has nevertheless enabled me to obtain the following results.

On looking through a plate of a crystal of Ludlamite parallel to a cleavage-plane in a Nöremberg's polarization-apparatus, one of the optic axes may be discerned near the edge of the field; and it is also seen that the optic axes lie in the principal section parallel to the plane of symmetry.

Accordingly two faces were ground on the last-mentioned crystal as nearly as possible parallel to the plane of symmetry. One of the planes thus ground was cemented to a plate of glass and laid upon the revolving stage of a microscope, and by the aid of a goniometer eyepiece, one wire of which was accurately adjusted parallel to the plane of polarization of the light employed, the directions were determined of the principal sections of the crystal relatively to the trace or edge of the cleavage-plane, 0 0 1. This process gave an angle of 12° 22′ for the inclinations of the two principal sections upon the trace of the face 0 0 1 and on its normal respectively, the greater angle of 102° 22′ for one of the principal sections lying on the positive†

* Supplementary notice to Mr. Field's memoir read before the Crystallological Society on December 15, 1876. Communicated by the Author.
† In my first communication the contour of the fragment led me to the erroneous conclusion that this angle was on the negative side of the normal.

side of the normal 0 0 1. Consequently the normal of the face 1 0 0 makes an angle of 22° 55′ on one of the principal sections. A plate was accordingly ground from this crystal in a direction as nearly perpendicular as was attainable to this principal section; and a second plate was formed from a cleavage fragment of another crystal as nearly as possible perpendicular in direction to that of the former plate and to the plane of symmetry.

Examined in oil, the optic axes were found to make an apparent angle of 97° 50′ in the former plate and of 119° in the latter. The first mean line is therefore inclined on the normal of the face 1 0 0 at 22° 55′, and on the normal of the face 0 0 1̄ at an angle 77° 38′; and the acute angle between the optic axes within the crystal, as obtained by calculation, is 88° 54′.

The dispersion of the optic axes for the different colours appears to be inconsiderable. The striated structure of the crystal from which the plate was cut perpendicular to the first mean line somewhat obscured the tints close to the hyperbolas; but in both the eyes representing the optic axes a blue fringe on the convex side of the hyperbola was discernible. In the other plate this characteristic was more marked, as in it each hyperbola presented a blue fringe within the first ring on the side nearest, and a red fringe on the side opposite, to the second mean line. Consequently the mean lines for these colours can be very little dispersed; for their dispersion is not recognizable in the plates experimented on; but the optic axes for the red rays present a slightly wider divergence in respect of the first mean line than do those for blue light.

The optical character is *positive*, as shown by the dilatation of the rings in the former of the two plates when a plate of quartz, cut perpendicular to the axis, is rotated on a line perpendicular to that which joins the optic axes in a Nöremberg polarization-apparatus.

It may be worth while observing that the directions of the principal sections in a small plate can be determined with ease and with considerable accuracy by the method above described, since, if the point of maximum obscuration is difficult to be hit upon with precision, this determination can be much facilitated by a slight rotation of the analyzing prism to right or left; for then tints developed in the crystal assist in indicating a slight inclination of its principal section to the plane of polarization. Of course the relative directions of the principal sections of the crystal, and of the trace (or edge) of one of its faces perpendicular to the plane of symmetry, are readily determined either by the revolution of the crystal on the rotating stage till the trace of the plane is in contact with the wire of

the eyepiece, or by bringing the rotating wire of the eyepiece into apparent contact with the edge of the face in question.

The method is available and chiefly useful in cases where it is almost impossible, or at least requires a refined lapidary's skill to form an artificial twin according to the elegant method of Des Cloizeaux.

N.B.—As a contribution tending to complete my memoir of December 15, I add three more figures representing various forms of the crystals of Ludlamite.

Fig.3.

Fig.4.

Fig.5.
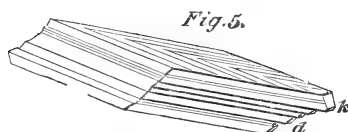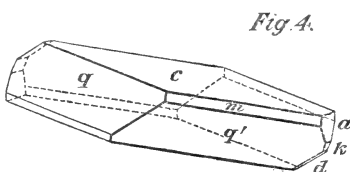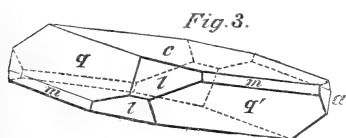
January 18, 1877.

XIX. *A Theorem in Continuants.*
By Thomas Muir, *M.A., F.R.S.E.**

IN the Philosophical Magazine for June 1853 (vol. v. p. 453) Professor Sylvester gave the first imperfect germ of a theory of continuants—determinants of the form

$$\begin{vmatrix} a_1 & b_1 & 0 & 0 \\ -1 & a_2 & b_2 & 0 \\ 0 & -1 & a_3 & b_3 \\ 0 & 0 & -1 & a_4 \end{vmatrix} \text{ or } K(a_1{}^{b_1} \ a_2{}^{b_2} \ a_3{}^{b_3} \ a_4)$$

—pointing out their connexion with continued fractions, as seen in the identity

$$a_1 + \cfrac{b_1}{a_2 + \cfrac{b_2}{a_3 + \cfrac{}{\ddots + \cfrac{b_{n-1}}{a_n}}}} = \frac{K(a_1{}^{b_1} \ a_2{}^{b_2} \dots a_n)}{K(a_2{}^{b_2} \dots a_n)};$$

and in the next volume (pp. 297–299) he established a funda-

mental theorem regarding them.   A full account of their pro-
perties (written without a knowledge of Sylvester's early dis-
covery) will be found in the 'Proceedings of the Royal Society
of Edinburgh' for the Session 1873–74.

The following new theorem on the subject seems to me in-
teresting, both on its own account and because of the exceed-
ingly simple way in which it is demonstrated.

Consider the continuant

$$\begin{vmatrix} a_1 + x & \omega + a_1 x & 0 & 0 & 0 \\ -1 & a_2 & \omega + a_2 x & 0 & 0 \\ 0 & -1 & a_3 & \omega + a_3 x & 0 \\ 0 & 0 & -1 & a_4 & \omega + a_4 x \\ 0 & 0 & 0 & -1 & a_5 \end{vmatrix}$$

Increasing the elements of the first, second, ... rows by x
times the corresponding elements of the second, third, ...
rows, it departs from the continuant form, becoming

$$\begin{vmatrix} a_1 & \omega + a_1 x + a_2 x & \omega x + a_2 x & 0 & 0 \\ -1 & a_2 - x & \omega + a_2 x + a_3 x & \omega x + a_3 x & 0 \\ 0 & -1 & a_3 - x & \omega + a_3 x + a_4 x & \omega x + a_4 x \\ 0 & 0 & -1 & a_4 - x & \omega + a_4 x + a_5 x \\ 0 & 0 & 0 & -1 & a_5 \end{vmatrix}$$

Now, however, diminishing the elements of the second
column by x times the corresponding elements of the first
column, diminishing the elements of the third column by
x times the corresponding elements of the *new* second column,
and so on, we have

$$\begin{vmatrix} a_1 & \omega + a_2 x & 0 & 0 & 0 \\ -1 & a_2 & \omega + a_3 x & 0 & 0 \\ 0 & -1 & a_3 & \omega + a_4 x & 0 \\ 0 & 0 & -1 & a_4 & \omega + a_5 x \\ 0 & 0 & 0 & -1 & a_5 + x \end{vmatrix}$$

which again is a continuant; hence the theorem

$$K\left(a_1 + x^{\omega + a_1 x} a_2^{\omega + a_2 x} \ldots a_n\right) = K\left(a_1^{\omega + a_2 x} a_2^{\omega + a_3 x} \ldots a_n + x\right).$$

Glasgow, January 20, 1877.

XX. *A Sensitive Mercury Barometer.*
By FREDERICK GUTHRIE[*].

THE object aimed at is to get a barometer which shall combine great sensibility with sufficient compactness. The barometers depending upon the weight of liquid columns are, unless the liquid is mercury, of unwieldy length. Descartes suggested a modification of the mercurial barometer in which the air-supported column of liquid consists of two liquids, the lower one being mercury and the upper one water holding tartar-emetic in solution to ensure the expulsion of air (fig. 1).

Fig. 1.

Now that we know various liquids, notably hydrocarbons, which have no sensible vapour-tension at atmospheric temperatures, it is surprising that this form has not been reintroduced. It appears, however, to be little known. It was suggested to me by a friend; and I made one containing glycerine and used it with success for some months before I was aware of its having been suggested by Descartes. The sensibility of such a barometer would obviously be, if the upper liquid were without weight, directly proportional to the ratio between the sectional areas of the cylindrical chamber and the upper tube (if also the open limb were of infinite area). But, the upper liquid having weight, the limit of sensibility is the comparative density of the mercury and liquid (say 16:1). Accordingly this limit is reached when the cylindrical chamber has four times the diameter of the upper tube.

By inclining the top part of the supported column a theoretical increase of sensibility is obtained; but practically, on account of the dragging on the column and for other reasons, this device is not in use for exact measurements. The multiplication of motion caused by applying a float connected with a wheel, as in the common weather-glass, is for similar reasons to be discarded for such purposes. Aneroid barometers are of exceedingly convenient form, but are of course saddled with the objection that the metal chamber is never perfectly elastic, so that they require frequent comparison with standard mercurial ones.

My friend the late B. F. Duppa devised a barometer in which the cistern or open limb was dispensed with, and was replaced by an open horizontal capillary tube. The free sur-

face of the mercury in the capillary tube would then move at a rate proportional to the sectional areas of the vacuum end and the capillary. I believe this plan was not found to answer, on account of the oxidation of the free end of the mercury.

The plan which I submit, and which in the models I have made and have had made appears to succeed well, more resembles that of M. Duppa than of any other with which I am acquainted. One form, shown in fig. 2, made for me by Messrs. Cetti, of Brooke Street, Holborn, consists of an ordinary barometer-tube, B, 6 millims. in internal diameter, connected by a flat horizontal spiral, S, of 2 millims. internal diameter, with the open tube C of the same diameter as B. In S is a bubble of air, *a*, at such a distance from B that it cannot enter B when the mercury in that tube is at the top. The motion of the bubble is, of course, 9 times as fast as that of the level of the mercury in either limb, or 4½ that of the true barometric variation. The object of bringing B and C together is to avoid as much as possible the effect of the relative differences of height on change of place. In another form (which is being made by Mr. Hicks, of Hatton Garden), the limb C is made to enclose hermetically the limb B. This should bring the effect of such relative tilting to a minimum.

Fig. 2.
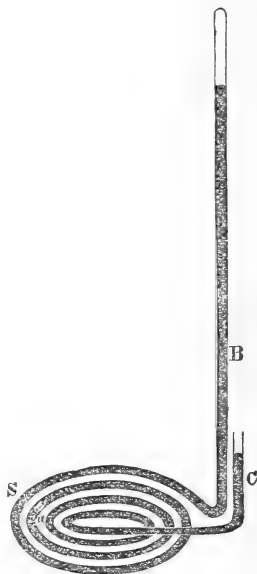
In one which I have myself made, the tube S being straight and placed on a stone mantlepiece, a drop of sulphuric acid is employed instead of the air-bubble. It has the effect of making the motion rather more nimble, but is perhaps not quite so trustworthy for exact measurements, because the wetting of the tube by the liquid causes some inconstancy in its calibre. A tube S of 2 millims. internal diameter, connected with a barometer-tube having at its upper mercurial surface a diameter of 20 millims., would exaggerate barometric motion 100 times ; and of course the sensibility would approach the nearer to twice this, the greater the diameter of the open limb. There is indeed absolutely no theoretical limit to the possible sensibility. It must be left to practical glass-workers to determine whether a spiral form is the best for the tube S, or whether a series of parallel tubes joined alternately end to end are to be

preferred.   According to Professor Dewar, who has had great experience in the use of such tubes for similar purposes, it would not be advisable to have the S tube less than 2 millims. internal diameter.   In all cases, on account of the unavoidable irregularities in so long a tube, I presume as many as possible comparisons should be made with a standard barometer and the intermediate graduations introduced by interpolation.   In the straight one mentioned above, the gap in the mercury thread is seen through a lens to be in constant agitation.

XXI. *Notices respecting New Books.*

The Vocabulary of Philosophy, Mental, Moral, and Metaphysical: with quotations and references for the use of Students.   By William Flemimg, *D.D., late Professor of Moral Philosophy in the University of Glasgow.   Third Edition.   Edited by* Henry Calderwood, *LL.D., Professor of Moral Philosophy in the University of Edinburgh.*   London: Charles Griffin and Company, Stationers' Hall Court. 1876. Crown 8vo. 540 pages.

THE object of this work is sufficiently described by its title-page. The editor justly remarks that the fact of its having "soon passed through two editions shows that it has supplied a want felt by those entering upon philosophic study."   It contains a number of short articles explaining and illustrating the meaning of about 800 words. The articles vary in length from six or eight lines to two or three pages.   In one case an article (that on *Idea*) extends to seven pages.   The exposition of the meaning of the words is mainly effected by illustrative extracts, a method which gives variety and interest to the discussion.   Indeed the book might be fairly called an entertaining book, in the way in which books of detached "Thoughts," or "Guesses at Truth," or "Table Talk" are entertaining.   Besides, words are used with different shades of meaning by different authors according to the cast of the opinions they have embraced; and this difference is best shown by actual quotation.

An account of a single article will sufficiently exhibit the method of the book; we will take that on *Experience*.   It consists, in the first place, of a statement that, "according to Aristotle (*Analyt. Poster.* ii. 19), from sense comes memory, but from repeated remembrance of the same thing we get *experience.*"   Wolf and Whately are quoted to bring out the meaning of the word more fully. Locke is then quoted to exhibit the view that experience is the sole source of knowledge; and Sewell for the opposite view, that men have knowledge *à priori*.   The article ends with quotations from Boscovich and Herschel, pointing out the distinction between experience derived from observation and from experiment.   This is the article as originally written by Dr. Fleming.   The editor, however, has inserted two paragraphs, which are distinguished by being

enclosed in brackets. The first defines the meaning of the word *experience*, viz. "Personal knowledge. It is identical with the facts of our consciousness." The second is an extract from Kant in illustration of the opinion that men have knowledge *à priori*.

A dictionary, as it consists of a number of separate articles, cannot be adequately criticised without going into details to an extent that our limits forbid. It will be enough to say that the articles for the most part are, as far as we can venture to judge, accurately and instructively written. In some cases attention is drawn to important distinctions which might easily be overlooked. Thus attention is drawn to the two meanings of the word *realism*, by giving a separate heading and article to each :—to *realism* as opposed to *idealism*, *i. e.* the doctrine that in perception there is an immediate cognition of the external object ; and to *realism* as opposed to *nominalism*, *i. e.* the doctrine that Genus and Species are real things, existing independently of our conceptions and expressions. Again, under the word *substance* attention is drawn to its twofold derivation : viz. from *subsistens* (*ens per se subsistens*), that which subsists of or by itself ; or from *substans* (*id quod substat*), that which lies under qualities.

In drawing up such a book as the present, one of the chief difficulties, we should suppose, is to determine what words are proper to be admitted and what to be excluded. We are inclined to think that this difficulty has not been very successfully dealt with in the present case. Thus, " of providence, foreknowledge, will, and fate," articles are allotted to providence, will, and fate, or at least fatalism, but none to foreknowledge. Again, there is an article on Gymnosophist, but hardly on Sophist ; so there is one on Manichean, but not on Gnostic ; and on Sabism but not on Buddhism. Still more strange is the insertion of so unusual a word as Hylozoïsm, and the omission of Evolution. This can only be due to an oversight ; for the author does not refuse to insert words of recent invention : *e. g.* he thinks it necessary to give a definition of such a phrase as " Endogamous Marriage," though it can have been used but rarely by any one but the author who invented it ; and, besides, it is quite on the borderland, if it be within the region, of the "Vocabulary of Mental, Moral, and Metaphysical Philosophy."

Dr. Calderwood's purpose in the present edition has been to keep the book " as nearly as possible in the form in which it came from the hands of Professor Fleming." He has, however, occasionally withdrawn quotations, introduced new matter from the author's notes, added a few new words, and, besides, "has ventured to introduce definitions of the leading vocables." The additions are placed between brackets ; but it is to be presumed that the editor is solely responsible for them only in the cases in which they are marked " Ed." The additions are of very unequal value : not unfrequently they distinctly improve the article in which they are inserted ; but sometimes they are unnecessary, and sometimes open to exception. Thus *credulity* is said to be " a disposition to believe [without evidence] what others tell us." The words within the brackets are

plainly unnecessary. A credulous man does not believe without evidence, *i. e.* without testimony; but his disposition inclines him to accept evidence, be it good or bad, without sifting it. Credulity, at all events in modern English, is lightness or easiness of belief. Again, the sentence in brackets setting forth the meaning of *obligation* is open to several adverse remarks. It stands thus:—"[Personal subjection to the authority of law—oughtness—duty. 'Bounden duty' is tautological.—Ed.]" This definition is far inferior to Dr. Johnson's, viz. Obligation is the binding power of an oath, vow, duty, or contract*. Then the attempt to throw light on the meaning of the word by saying that obligation is *oughtness*, is simply grotesque. Goodness we know, and badness we know, but what is oughtness? It seems (p. 142) that Price has used *oughtness* as synonymous with rightness. But something more than Price's authority is needed for proving a word from his mint to be English, and the more when it has every appearance of being wrongly formed, and is quite useless if rightly formed. Again, it is hard to see what could have induced Dr. Calderwood to insert the words " 'Bounden duty' is tautological." Supposing it to be so, there was no sort of need for saying it in what purports to be "a definition of the vocable" Obligation. Besides, tautology not only means repetition of the same sense in the same or different words, it also implies needless or inelegant repetition, and as such it is a fault of style. Of course in some contexts "bounden duty" might be a needless or inelegant repetition, but it is not so under all circumstances. In the language of passion or emotion repetition is often a most effective means of giving emphasis to a word or phrase. *E. g.* where Hamlet says

"O, that this too, too solid flesh would melt,"

there is repetition but not tautology; and the same is true when Macbeth says—

> "That but this blow
> Might be the be-all and the end-all here,
> But here, upon this bank and shoal of time,
> We'd jump the life to come."

And so the phrase "bounden duty," when used as it occasionally is in daily life, contains an emphasizing repetition, which cannot fairly be called tautological. At least this is certainly the case in the passage of the Communion service, from which, as we suppose, the phrase is derived, where we speak of "Our bounden duty and service." The fact is that very brief criticism is apt, as in the present instance, to be wanting in discrimination. Probably the form of the work exposed both author and editor to the temptation of settling points with more brevity than the case fairly admitted; and they have sometimes fallen into it. Thus the author gives a refutation of Manicheism in four lines and a half; the refutation could be easily met by a specious argument, and so is incomplete; while, in

* Of course the word has a secondary meaning, viz. an act binding a person to a certain performance.

a book such as the present, it was no part of the author's duty to refute objectionable doctrines. He does not, for instance, refute the doctrine of Metempsychosis ; he merely states what it was : and in like manner he lets Polytheism pass with nothing more than definition.

We had marked some other passages for comment ; but our limits are reached, and we will only add that our criticisms are not intended to show more than that, where much has been well done, something might have been done better.

XXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 73.]

May 18, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

THE following communication was read :—

"On Stratified Discharges.—II. Observations with a Revolving Mirror." By William Spottiswoode, M.A., Treas. R.S.

In a paper published in Poggendorff's 'Annalen,' Jubelband, p. 32, A. Wüllner has described a series of observations made, by means of a revolving mirror, upon the discharge of a large induction-coil through tubes containing ordinary atmospheric air at various degrees of pressure. When, as is generally the case with an induction-spark, the discharge occupies an appreciable interval of time, the image in the mirror appears spread out to a breadth proportional to the duration and to the velocity of rotation. The successive phases of the phenomena then appear, as usual, arranged in successive positions, and may be studied separately, even when too rapid to be disentangled by the unassisted eye.

Wüllner's observations appear to have been directed rather to the nature of the coil-discharge than to that of the stratifications ; and some of his principal conclusions are accordingly of the following kind, viz. that at low pressures, *i. e.* down to 1 millim., when the discharge was stratified, the striæ showed an intermittence of intensity, indicating either a pulsation within the duration of the main discharge or a breaking-up of the main into a series of partial discharges. At greater pressures, *e. g.* at 26 millims., when almost all trace of stratification was lost, this breaking-up into partial discharges (especially at the commencement) was distinctly perceptible. At yet greater pressures, *i. e.* from 40 millims. to 75 millims., a cloudy kind of stratification showed itself; but, excepting a bright flash at the outset, no appearance of partial discharge was visible. The observations, which were at first directed to capillary tubes, were extended to tubes of various diameters, and also included the effect of a magnet on the discharge.

For some time prior to the publication of the volume in question I had been engaged upon a series of experiments very similar in their general disposition, but with a somewhat different object

in view, viz. the character and behaviour of the striæ; and of these, together with some recent additions, I now propose to offer a short account to the Society.

My general instrumental arrangements appear to have been similar to those of Wüllner; in fact they could hardly have been very different. The tubes were attached to the coil in the usual way, and a contact-breaker of the ordinary form with its own electromagnet was in the first instance used. By suitably adjusting the velocity of the mirror to the rapidity of the contact-breaker the image could be kept tolerably steady in the field of view. In order to obtain greater steadiness a special contact-breaker was next devised. This was mechanically connected with the spindle of the mirror, and so arranged as to break the current when the image was in the centre of the field of view. The only point in this part of the apparatus which requires special notice is the fact that this contact-breaker, like all others, should be placed in close proximity to the condenser of the coil, otherwise a great loss of light is sustained. For the last-mentioned form there was finally substituted a mercurial break (successfully arranged by my assistant, Mr. Ward), the plunger of which works on a cam attached to the axle of the mirror; so that the action of the contact-breaker is regulated by that of the mirror, instead of the reverse as in the former arrangement. With the broader tubes a slit was used; with the narrower this adjunct was less necessary; while with capillary tubes, such as are used for spectrum-analysis, it could be dispensed with altogether.

In experiments for comparing the unstratified statical discharge with the stratified at the same pressure of gas within the tube, and for observing the transition from one to the other, a Leyden jar and a spark of air, the length of which could be regulated at pleasure, were introduced into the secondary circuit.

Striæ as observed by the eye have been divided into two classes, viz. the flake-like and the flocculent or cloudy. Of the former those produced in hydrogen-tubes may be taken as a type; of the latter those produced in carbonic-acid tubes. But upon examining some tubes especially selected for the purpose, it was found that, while to this apparent difference a real difference corresponds, a fundamental feature of the striæ underlying both was brought out.

The feature in question was this—that the striæ, at whatever points produced, appear to have generally during the period of their existence a motion along the tube in a direction from the negative towards the positive terminal. This motion, which I have called, for convenience, the proper motion of the striæ, is for given circumstances of tube and current generally uniform; and its variations in velocity are at all times confined within very narrow limits. The proper motion in this sense appertains, strictly speaking, to the flake-like striæ only. The apparent proper motion of the flocculent striæ is, on the contrary, variable not only in velocity, but also in direction; and on further examination it turns out that the flocculent striæ are themselves compounded

of the flake-like, which latter I have on that account called elementary striæ.

Elementary striæ are in general produced at regular intervals along the tube. The series extends from the positive terminal in the direction of the negative to a distance depending upon the actual circumstances of the tube and current. The length of the column, and consequently the number of the striæ, depends mainly upon the resistance of the tube, the duration of the entire current, and, to a certain extent, upon the amount of the battery-surface exposed; and in that sense upon the strength of the current. The velocity of the proper motion, other circumstances being the same, depends upon the number of cells employed; in other words, upon the electromotive force.

The appearances of the striæ, however, their essential features, and the conclusions which may be drawn from them will be better apprehended by means of sketches, even though imperfect, than by meredescription; and I therefore subjoin a few examples.

Fig. 1 represents the appearance of (in the mirror) a carbonic-

Fig. 1.
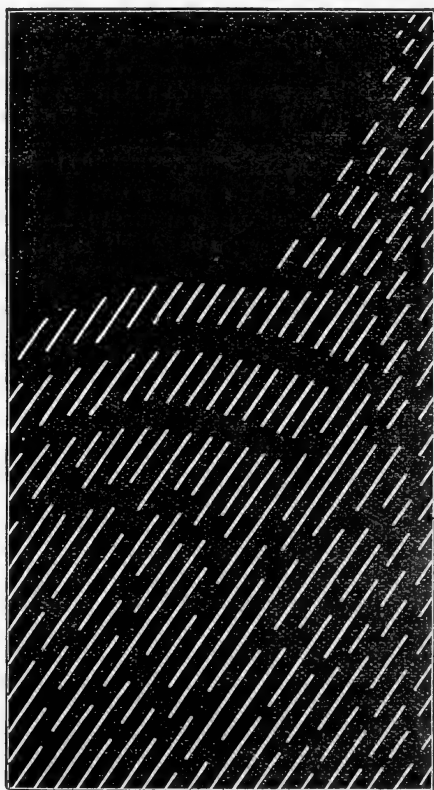
acid tube with the slit attached. This tube, viewed by the eye, shows flake-like fluttering striæ, with a slight tendency to flocculency near the head of the column. The commencement of the discharge is at the right hand, and the negative terminal at the top. The drawing fairly represents the appearance of the upper part or head of the column of striæ during one complete coil-discharge. When the battery-surface exposed is small, the whole consists of, first, three or four columns of striæ of decreasing length, and afterwards of an almost unbroken field of striæ. Each of the initial columns is perfectly stratified; and the same disposition of striæ prevails throughout the entire discharge. The striæ which fill the main part of the field present a proper motion nearly uniform, but slightly diminishing towards the end. These striæ are for the most part unbroken, but are occasionally interrupted at apparently irregular intervals. When the battery-surface is increased the elementary striæ are more broken, and near the head of the column the interruptions occur as in the figure. The separation of the earlier part of the discharged into striated columns divided by intervening rifts does not, with the exception of the first, extend far towards the positive terminal. Nevertheless, even as far as the positive terminal itself, there seems at times to be a fuller development of discharge than is subsequently maintained.

The first rift in the discharge, following the first outburst, is sometimes distinguishable even as far as the positive terminal, and, perhaps, in those cases indicates a real cessation of the discharge. This is corroborated by the fact that a similar interruption is then perceptible in the glow surrounding the negative terminal; but after this the negative glow retains its unbroken character throughout the entire period of the discharge.

The stratified columns with their intervening rifts are sometimes reproduced towards the close of the discharge; but this appears to take place only when the battery is in an unusual condition of energy, and disappears when, as in the bichromate battery, polarization of the plates rapidly takes place. On these occasions especially, but also at other times, traces may be seen of the faint lines of light connecting the positive with the negative parts of the discharge mentioned by Wüllner in the paper quoted above.

Other tubes, when viewed by the eye, show flaky striæ more or less difficult to distinguish from one another. Observed in the mirror, they show much the same phenomena as the tube figured above, except that the striæ are rather more crowded together and slightly more broken. This is the case especially with ammonia-tubes, in some of which the striæ are undistinguishable by the eye, and which accordingly give the impression of an unstratified column of light.

Fig. 2 represents the appearance in the mirror of another carbonic-acid tube with a current similar to that used in the former case. Viewed by the eye it shows flocculent striæ, each having a contour sharply defined towards the negative terminal, loosely de-

fined towards the positive. The following description of the phe-
nomena, taken from my earlier notes, may now be regarded as a

Fig. 2.
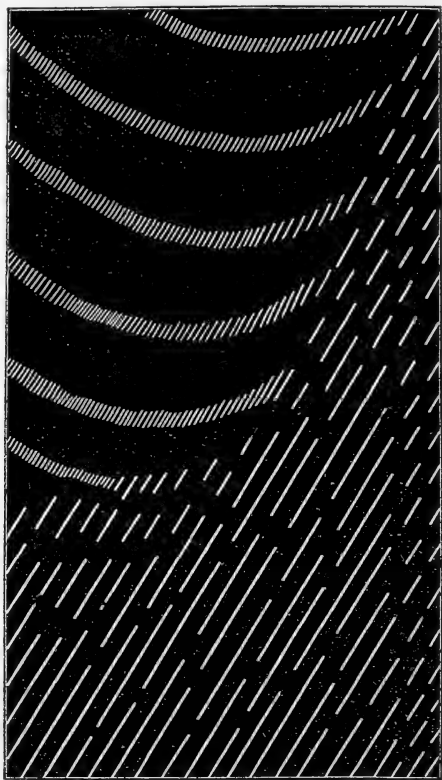
description of the apparent proper motion of the flocculent as
distinguished from the elementary striæ :—" The discharge opens
with a considerable rush, indicated by the bright line at the com-
mencement. There is no other indication of partial discharges.
Proper motion at first towards the negative, afterwards towards
the positive terminal. In this, as in other tubes giving striæ of
this kind, ripples may be observed on the curve of proper motion."

So far my older notes; but on closer examination, and when
the battery-surface exposed is sufficiently reduced, the entire field
is seen to be traversed by elementary striæ having a normal proper
motion. When the battery-surface is gradually increased, the
elementary striæ, especially near the head of the column, have
their duration shortened so as to leave dark intervals at regular
stages in the column. These successive short-lived elementary striæ
form a series of diagonal lines, each series of which traces a
sketch of a flocculent stria. As the surface is still further in-

creased these diagonal lines appear more and more crowded together, until at last they blend into unbroken flocculent striæ.

This compound nature and mode of formation may be taken as a general characteristic of the flocculent striæ. In some tubes it is more easily brought out, in others only with greater difficulty. In some it can hardly be verified experimentally without a loss of light so great as to mask the phenomenon. The apparent proper motion of the flocculent striæ depends, as is easily seen, upon the position at which the elementary striæ are replaced. If they are replaced in the positions which their predecessors held, the flocculent striæ will appear straight in the mirror; if they are replaced successively nearer the positive terminal, the apparent proper motion will be in the normal direction; if nearer the negative, it will be reversed.

An ether-tube examined in the same way showed nearly the same features as the last. The elementary striæ, however, were not so easily separable; and the flocculent striæ were formed, as usual, at an earlier stage near the head of the column than near the foot of it.

In another carbonic-acid tube the proper motion of the flocculent striæ was coincident in direction with that of the elementary; and the latter were consequently more difficult to disentangle. One point in this tube was particularly noticeable, viz. that as the column of flocculent striæ retreated, so did the negative glow advance. The two remained throughout the entire discharge the same distance apart.

Fig. 3 represents the discharge in a hydrogen-tube of conical

Fig. 3.
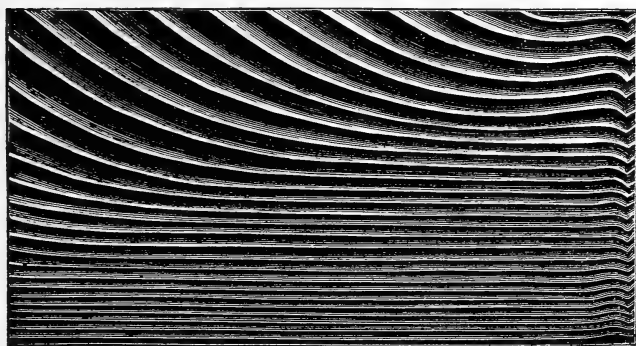
form, the diameter of which varied from capillary size to $\frac{1}{2}$ inch, the capillary end being at the bottom. The positive terminal is at the top. The principal interest of this tube consists in showing the influence of diameter upon the velocity of proper motion. The wider the tube the freer, it seems, the striæ are to move. The same fact may be observed by comparing tubes differing in diameter, but in other respects the same; but the conical tube brings out the fact in the most striking manner.

Fig. 4 represents a chloroform-tube, in which a piece of cotton-wool had been inserted with the view of ascertaining whether any

Fig. 4.
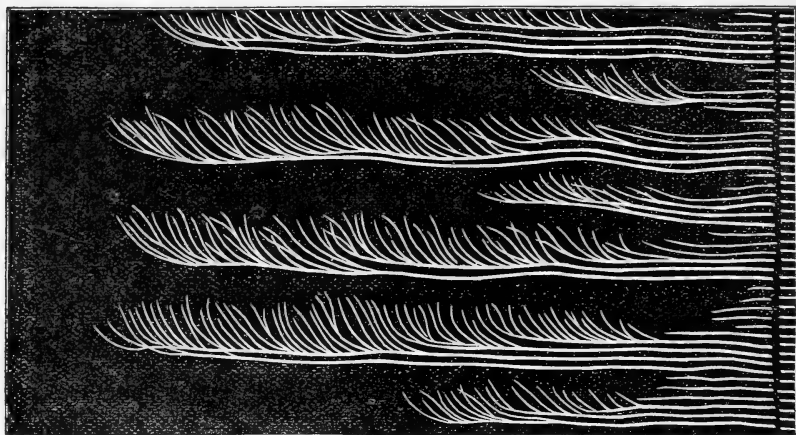
motion would be communicated to it by the current.   This proved to be the case; but I do not attempt here to describe the phenomenon.   To the unassisted eye the discharge was extremely brilliant; it passed in a column not quite straight, but in a writhing snake-like curve, with flaky striæ at intervals through its length.   When viewed in the mirror the striæ were seen to spread themselves out with slight, but irregular, proper motion. With an increased battery-surface, or with a greater number of cells, but more notably with the latter, not only were the striæ lengthened, but from several of the long elementary striæ shorter ones were thrown out nearly at right angles to the former.   These were of short duration, and had great proper motion. The general appearance of these compound striæ was that of branches of fir trees, the twigs of which represented the permanent striæ, and the leaves the secondary.

Beside these, a large (Geissler's " hydrocarbon ") tube was examined with a magnet the pole of which was placed near the head of the column; and in order to trace more in detail the effect of the magnet, its strength was varied by raising or lowering the battery-plates.   The general character of the discharge without the magnet was very similar to that represented in fig. 1.   On slightly lowering the plates of the magnet-battery the discharge spread itself over a greater breadth than before.   At the same time the elementary striæ, which had for the most part been continuous, were now broken up into short lengths, presenting the first features of flocculent striæ.   On further lowering the plates these flocculent striæ became more and more developed until the whole field in the neighbourhood of the magnetic pole became filled with such striæ.   It is well known that one effect of the magnetic field is to bring out striæ in portions of tubes where no striæ

were visible before, and also that the striæ so brought out present a flocculent appearance; but the revolving mirror shows this fact in a more decisive manner. Another effect of the magnetic field is to drive the discharge to one side of the tube in accordance with Ampère's law—in other words, to constrict the discharge. In narrower tubes than the one here described, the constriction goes so far as to imitate the appearance of a capillary tube; and this effect is borne out by the revolving mirror. The intensification of the discharge and its concomitant phenomena within the range of the magnetic field are in accordance with the experiment of Faraday, wherein he showed the increased loudness of the report perceptible on breaking a current in between the poles of a magnet.

In a carbonic-acid tube (Gassiot's No. 454) I have succeeded in starting with a very weak current, capable of producing only elementary striæ, and thence passing to the production of flocculent striæ, either by strengthening the battery-current, or by inducing upon the existing current the action of a magnetic field. The identification of the results of these two independent processes, especially when combined with the comparison made above of the effect of magnetism with that of narrowing the tube, can hardly fail to have some important signification in the ultimate theory of the striæ.

Besides the tubes above mentioned many others were tried; but these will probably suffice for the present purpose.

The following are some of the general conclusions to which the foregoing experiments seem to lead :—

I. The thin flake-like striæ, when sharp and distinct in their appearance, either are short-lived or have very slow proper motion, or both.

II. The apparent irregularity in the distribution of such striæ, during even a single discharge of the coil, is due, not to any actual irregularity in their arrangement, but to their unequal duration and to the various periods at which they are renewed. These striæ are, in fact, arranged at regular intervals throughout the entire column. The fluttering appearance usually noticeable is occasioned by slight variations in position of the elementary striæ at successive discharges of the coil. With a view to divesting the coil-discharge of this irregular character, as well as for other purposes, I devised two different forms of contact-breakers (one of which is described in the Royal Society's 'Proceedings,' vol. xxiii. p. 455); but I postpone a description of the second, as well as of the experiments arising from its use, to another occasion.

III. The proper motion of the elementary striæ is that which appertains to them during a single discharge of the coil. This appears to be generally directed from the positive towards the negative terminal. Its velocity varies generally within very narrow limits. It is greater the greater the number of coils employed, or the greater the electromotive force of the current. In some tubes it may be seen to diminish towards the close of the discharge; and even in rare instances alternately to increase and to diminish during a single discharge.

IV. Flocculent striæ, such as are usually seen in carbonic-acid tubes, are a compound phenomenon. They are due to a succession of short-lived elementary striæ, which are regularly renewed. The positions at which they are renewed determine the apparent proper motion of the elementary striæ. If they are constantly renewed at the same positions in the tube, the flocculent striæ will appear to have no proper motion and to remain steady. If they are renewed at positions nearer and nearer to the positive terminal, the proper motion will be the same as that of the elementary striæ; if they are renewed at positions further and further from the positive terminal, the proper motion will be reversed.

V. The velocity of proper motion varies, other circumstances being the same, with the diameter of the tube. This was notably exemplified in the conical tube. In tubes constructed for spectrum-analysis the capillary part shows very slight, while the more open parts often show considerable proper motion.

VI. Speaking generally, the discharge lasts longer in narrow than in wide tubes. In spectrum-tubes the capillary part gives in the mirror an image extending far beyond that due to the wider parts.

VII. The coil-discharge appears, in the earlier part of its development at least, to be subject to great fluctuations in extent. In all cases there is a strong outburst at first. This, although sometimes appearing as a bright line, is always, I believe, really stratified. Immediately after this there follows a very rapid shortening of the column. The extent of this shortening varies with circumstances; but when, as is often the case, it reaches far down towards the positive terminal, a corresponding diminution of intensity is perceptible in the negative glow. The column of striæ, after rising again, is often subject to similar fluctuations. These, which are sometimes four or five in number, are successively of less and less extent, and reach only a short distance down the column of striæ. The rifts due to these fluctuations then disappear, and the striæ either continue without interruption, or follow broken at irregular intervals, until the close of the discharge.

VIII. The effect of the proper motion, taken by itself, is to shorten the column of striæ. But, as we have seen, the striæ are in many cases renewed from time to time. In regard to this point, the head of the column presents the most instructive features. After the cessation of these rifts, the general appearance of the field is that of a series of diagonal lines commencing at successive points which form the bounding limit of the column at successive instants of time. If the points are situated in a horizontal line, the striæ are renewed at regular intervals at the same place; and the length of the column is maintained by a periodic renewal of striæ, a new one appearing at the head of the column as soon as its predecessor has passed over one dark interval. If the boundary of the illuminated field rises, the length of the column increases; if it descends, the column shortens. In every case, however, the growth of the column takes place by regular and successive steps, and not irregularly. The intervals of

the new striæ from one another and from the old ones are the same as those of the old ones from one another.

IX. The principal influence of a change in the electromotive force appears to consist in altering the velocity of proper motion. A change in the amount of battery-surface exposed produces a corresponding change in the duration of the entire discharge, as well as, apparently, in the development of some of the minor details of the striæ.

X. When the proper motion of the elementary striæ exceeds a certain amount, the striæ appear to the eye to be blended into one solid column of light, and all trace of stratification is lost. When this is the case the mirror will often disentangle the individual striæ. But there are, as might well be expected, cases in which even the mirror is of no avail, but in which we may still suppose that stratification exists. A variety of experiments have led me to think that the separation of the discharge into two parts, viz. the column of light extending from the positive terminal, and the glow around the negative, with a dark space intervening, may be a test of stratified discharge; but I cannot affirm any thing certainly on this point.

GEOLOGICAL SOCIETY.

[Continued from p. 75.]

December 20th, 1876.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

The following communications were read :—

1. " On *Pharetrospongia Strahani*, a fossil Holorhaphidote Sponge from the Cambridge Coprolite Bed." By W. J. Sollas, Esq., B.A., F.G.S.

2. " On the Remains of a large Crustacean, probably indicative of a new species of *Eurypterus*, or allied genus (*Eurypterus? Stevensoni*), from the Lower Carboniferous series (Cement-stone group) of Berwickshire." By Robert Etheridge, jun., Esq., F.G.S., Palæontologist to the Geological Survey of Scotland.

3. " On the Silurian Grits near Corwen, North Wales." By Prof. T. McKenny Hughes, M.A., F.G.S.

The author commenced with a description of sections near Corwen in North Wales, from which he made out that the grits close to Corwen were not the Denbigh grits, but a lower variable series, passing in places into conglomerate and sandstone with subordinate limestone and shale. This series, under the name of ' *The Corwen Beds*,' he described in detail, having traced them round the hills S. of Corwen, also near Bryngorlan, S. of the Vale of Clwyd, on Cyrnybrain, and S. of Llangollen. He had noticed in places a kind of double cleavage affecting the lower series but not the upper, and also fragments of cleaved mudstone included in the upper, from which he inferred a disturbance of the older rocks previous to the deposition of the newer. He exhibited a selection of fossils, and said that immediately below the Corwen beds there were none but Bala fossils. In the Corwen beds all the few fossils found were common to the Llandovery rocks, some of them, as *Meristella*

crassa and *Petraia crenulata*, being peculiar to that formation.   In the flaggy slates above the Pale Slates he had found Graptolites and Orthoceratites of the same species as those found in the Denbigh Flags.   He considered that the Corwen Beds were on the horizon of the May-Hill or Llandovery group, and should be taken as the base of the Silurian, thus including in the Pale Slates or Tarannon Shale a thick series which intervened between the Corwen Beds and the flaggy slates of Penyglog.

4. " On Mineral Veins."   By W. Morgan, Esq.

The author maintained that no one theory can be accepted in explanation of the formation of mineral veins, and that, whilst in some cases their formation may be due to the presence of preexistent fissures induced by shifting of the containing rock, in others any such explanation is insufficient, as he thought the means by which the sides of such fissures were kept apart could not be easily indicated.   The point upon which he especially insisted in connexion with this question was the presence of " horses " in many mineral veins.   He advocated the view that the walls of veins were in close proximity in their earliest stage, and that the enlargement and infilling of the veins took place simultaneously by the segregation of materials derived from the adjacent rock, supplemented, perhaps, by a tension or tendency to separation caused by slow contraction of the latter.   Instead of a fissure, he assumed the presence of an irregular surface of least resistance or of electrical action, at which the vein-matter might collect at first as a mere film.   In this way, he thought, the vein might increase and its walls might recede simply by the aggregation of the vein-matter itself, and in general in proportion to the degree of mineral saturation of the adjacent rocks.

January 10th, 1877.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

The following communications were read :—

1. " On gigantic Land-Tortoises and a small Freshwater Species from the ossiferous caverns of Malta, together with a list of the fossil Fauna, and a note on Chelonian remains from the Rock-cavities of Gibraltar."   By A. Leith Adams, Esq., M.B., F.R.S., F.G.S., Professor of Zoology in the Royal College of Science, Dublin.

2. " On the Corallian Rocks of England."   By the Rev. J. F. Blake, M.A., F.G.S., and W. H. Hudleston, Esq., M.A., F.G.S.

The object of the paper was to describe the rock masses existing betwixt the Oxford and Kimmeridge Clays.   Topographically the Corallian region is divided into five districts of very unequal size, wholly separated from each other.   The special features of each district were detailed, both as regards the development, composition, and fauna of the several subformations therein contained, and these latter compared with their equivalents or representatives in the other districts.   The old names were, as far as possible, retained ; but where obviously inapplicable, local names replaced them.

In the Weymouth district (I.) one section discloses 230 feet of beds between the Oxford and Kimmeridge Clays, made up in ascending order of grits, clays, marls, and oolites, gritty limestones very fossili-

ferous towards the top, clays and grits. Another section, on the oppo-
site side of the anticlinal, shows the same development of the central
limestones ; but the lower series is considerably attenuated, and the
upper series (Supra-Coralline) shrunk from about 90 feet to a thin
ferruginous band of only a few inches. There are hardly any
corals, and no Coral Rag whatever ; argillaceous and arenaceous
matter (always, however, more or less mixed with lime) prepon-
derates ; but there is a rich and varied fauna, which has strong
affinities with some of the Coralline beds of other districts. This
culminates in the *Trigonia*-beds, which lie towards the top of the main
limestone series ; above this the fauna inclines to Kimmeridgian,
below to Oxfordian types. The remarkable irregularity of the
Supra-Coralline beds was noted, especial reference being made to the
mineral character, fossil contents, and geological position of the
Abbotsbury iron-ore. In the North-Dorset district (II.) the thick-
ness of the mass is much reduced, and its constitution greatly
altered. Corals are still very rare ; but calcareous sediment greatly
preponderates, and is made up largely of comminuted shells,
loosely aggregated pisolites, and rubble frequently false-bedded.
The arenaceous base of the Corallian series, described generally
under the term Lower Calcareous Grit, is almost at its minimum in the
neighbourhood of Storminster. The central limestones contain a
moderate assemblage of the usual Coralline forms, but *Cidaris
florigemma* appears confined to a rubbly bed about 8 feet thick.
The West-Midland range (III.), extending from Westbury to Oxford,
exhibits the greatest variety, and, being classic ground, contains a
larger proportion of the type forms of the rocks. The development
is very unequal ; and the entire group is reduced to less than 25 feet
in some places ; but where the sandy base is expanded, as in those
districts where the escarpment faces the north, the thickness ex-
ceeds 100 feet, occasionally falling to about 30 feet in the direction
of the dip, with the probability of the entire mass ultimately
thinning to a feather edge. In many places true Coral Rag is
largely developed, usually terminating the Corallian series in an
upward direction, or at most succeeded by a very few feet of ferru-
ginous sand. Throughout the great escarpment facing the upper
valley of the Thames the lower arenaceous member predominates,
though much mixed with thin-bedded sandy clays, the whole con-
stituting a loose formation, which is capped by hard gritty lime-
stone containing an abundant fauna, representative of the middle
series, differing somewhat, on the one hand, from the Rag with
its partially Kimmeridgian character, and, on the other, from
the Lower Calcareous Grit, whose affinities are, of course, Ox-
fordian. The beds of this district, however, are so varied that
it is impossible to deal with them in an abstract. District IV.
includes the Coral reef at Upware, 75 miles E.N.E. of Oxford ;
though the exposures are small, they are very suggestive. The
limestone of the south pit is an excellent Coral Rag, but softer and
more chalky than much of the Coral Rag of the West-Midland
district. Moreover, whilst the rock contains many familiar forms,
and especially *Cidaris florigemma*, whose presence in abundance

invariably indicates a distinct horizon, we also find the casts of shells, rarely or never met with in the West of England, but which appear common in some parts of the Continent—*e. g.* species of *Isoarca*, a certain form of *Opis*, which latter occurs also in a portion of the Yorkshire Basin (V.). This bears 130 miles N. by W. from the reef at Upware. The Corallian beds are grouped as a belt of rocks enclosing an oval tract of Kimmeridge Clay. There is more symmetry here than in the south; and the triple division of grit, limestone, and grit, though not absolutely true in all places, is fairly accurate; most of the beds are better-developed, and the contrast between the Coral Rag and underlying Oolites is strongly marked. In the Tabular Hills these Oolites constitute a double series, divided by a " Middle Calc Grit," a fact first indicated on stratigraphical grounds by Mr. Fox Strangways, and amply borne out by fossil contents. The shell beds of the Lower Limestones are, especially in their lower parts, charged with Brachiopoda and other forms of the Lower Calc Grit; whilst the Upper Oolite, on which the Coral Rag reposes, contains a far more varied fauna, though singularly destitute of Brachiopoda. The fauna of the Rag here, as elsewhere, inclines to Kimmeridgian types.

As the object of the paper was to arrange facts rather than to propound theories, the conclusion was chiefly occupied in summing up and correlating. It was shown that, since the leading feature of the rock masses between the Oxford and Kimmeridge Clays is *variety,* a strict and rigid correlation is altogether impossible. Yet, in spite of great local differences, producing in many places a strangely contrasted facies, there are certain features which may be deemed fairly characteristic of the several divisions. The bank-like character of most of these beds was insisted upon. A table of comparative sections, 14 in number, affording a generalized idea of the development, was exhibited, and the stratigraphical verifications of many of these given, as sections drawn to scale, in the body of the paper.

XXIII. *Intelligence and Miscellaneous Articles.*

NOTE ON MR. LODGE'S PAPER "ON A MECHANICAL ILLUSTRATION OF THERMO-ELECTRIC PHENOMENA." BY PROF. M. AVENARIUS.

THE 'Philosophical Magazine' for 1876, No. 14, contains a treatise by Mr. Oliver J. Lodge, in which (p. 543) the author says that the electromotive force of a thermo-element is expressed by the formula $\qquad E \eqsim y(\theta_1 - \theta_2)\{\theta_0 - \tfrac{1}{2}(\theta_1 + \theta_2)\}$, ... (1)

in which y represents a constant, θ_1 and θ_2 the temperatures of the junction-places, and θ_0 the temperature of the neutral points. He here remarks, " The law was originally given nearly in this shape by Avenarius; but he omitted the two Thomson effects, and consequently his formula was erroneous " (here Mr. Lodge cites Pogg. *Ann.* vol. cxix. 1863).

In the first place, the expression which I used respecting the electromotive force of a thermo-element is not approximately re-

presented by the form (1); but the equation (1) completely represents the formula given by me*. If, therefore, Mr. Lodge considers the equation (1) correct, I really cannot understand how mine can be erroneous.

As to the point of view from which I tried to explain the origin of the empirical formula more than thirteen years ago, the latter, after the results of the experiments of Le Roux were added to those of Thomson, was subject to changes—a fact which I positively expressed in my paper, " Ein Beitrag zur Theorie der Thermoströme."

In 1870 I first gave a theoretical proof of the truth of my empirical formula (equation 1) in the ' Reports ' of the University of Kiew, and another in 1873, in Pogg. *Ann.* vol. cxlix. If Mr. Lodge had been acquainted with these memoirs, he certainly would not have reproached me with not taking notice of Thomson's two effects.

ON AN ARRANGEMENT FOR REPRODUCING, WITH THE AID OF THE SIREN, THE EXPERIMENT OF FOUCAULT (ARREST OF A ROTATING DISK UNDER THE ACTION OF AN ELECTROMAGNET). BY M. BOURBOUZE.

A well-known experiment of Faraday consists in placing between the poles of a powerful electromagnet a cube of copper or silver suspended at the end of a strongly twisted string ; the cube, left to itself, having taken a movement of rapid rotation, if a current be thrown into the electromagnet, the cube stops almost instantaneously.

Foucault realized a similar effect on causing a copper disk to rotate between the poles of an electromagnet. The mechanism he employed to produce the rotation was that which had served him for putting his gyroscope in motion.

The arrangement which I have adopted in order to repeat these experiments permits one to render sensible to a large audience the action of magnetism on a rotating disk. I fix, upon the axis of a siren, a disk of copper parallel to the movable plate, and rotating between the armatures of an electromagnet which may be carried by the siren itself. To set the siren in motion, I utilize the air-impelling apparatus which serves me for most of my acoustic experiments. The apparatus once started, and the sound produced being the sharper the greater the velocity of the rotation, the current is caused to pass into the electromagnet: the plate is arrested, and the sound, till then perceptible at a great distance, entirely ceases.

I ask permission to give on this occasion some account of the apparatus which I substitute for the bellows usually employed to set the siren going. It consists of a reservoir of compressed air of 50 litres capacity, put into communication with a pipe the extremity of which is very fine, and which penetrates the axis of a much larger conical tube. In the outer tube, as in the Bunsen

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* My formula is $E = (T_1 - T_2)(b + c(T_1 + T_2))$, whereby $\frac{b}{2c} = -T_0$ represents the temperature of the neutral points.

burner, apertures have been bored in order to produce a draught of air. This arrangement permits higher sounds to be obtained than with the ordinary bellows. A manometer, communicating with the entry-tube, indicates the pressure which corresponds to a determinate sound.—*Comptes Rendus de l'Académie des Sciences,* Dec. 18, 1876, p. 1235–36.

OBSERVATIONS ON A PROPERTY OF THE RETINA, FIRST NOTICED BY TAIT. BY OGDEN N. ROOD, PROFESSOR OF PHYSICS IN COLUMBIA COLLEGE.

In the Edinburgh ' Proceedings,' 1869–70, vol. vii. pp. 605–607, Tait described an interesting observation which has perhaps some bearing on Thomas Young's theory of colour. While suffering from indisposition, he noticed each time, on awakening from a feverish sleep, that the flame of a lamp seen through a ground-glass shade assumed a deep-red colour, the effect lasting about a second. He suggests that the nerve-fibrils in the retina also partook of sleep, and, on awakening, the green and violet nerves resumed their function somewhat later than the red. I have in my own case noticed some instances which seem to point out that after a *nervous shock,* sudden or prolonged, the green nerves (adopting the theory of Young) recover their activity later than the red, and probably later than the violet nerves. The first observation was made twenty years ago, while recovering from the effects of chloroform which had been administered by a dentist well known at that time in Munich. Upon regaining consciousness and raising my eyes to the face of the operator, I was a little surprised at not having previously remarked his unusually ruddy complexion, but the next instant saw that this was due to an optical illusion; for his hair appeared of a bright purplish-red hue. The singular appearance lasted perhaps a couple of seconds, when his hair resumed its natural colour, which was *white.* This observation corresponds with that made by Tait.

I give now an instance where chronic effects of a similar character were noticed by me for a couple of weeks continuously during convalescence from typhoid fever. In this case white objects appeared of a not very intense orange-yellow hue, the general effect on a landscape being such as is produced by the orange-yellow rays of the setting sun. Here the activity of the green and violet nerves was diminished relatively to that of the red. The auditory nerve was also evidently affected during the same period; but precisely in what way I did not ascertain.

It is a mttter of yearly observation with me, that effects similar in kind to those first noticed are produced by long exposure to bright white light out of doors. Under such circumstances, white objects no longer appear pure white, but are tinted plainly purplish-red; and rather dull greens assume a grey hue, as though all the green in them had been neutralized, while strong greens are considerably reduced in intensity (saturation). Upon leaving the blinding glare and entering a darkened room, it often for several seconds appears filled with a greenish haze.

Two of these cases, and probably that of Tait, point out that our

apparatus for the reception of waves of light of medium length is more liable to be overstrained by nervous shocks or by prolonged excitation than is the case with those designed for the reception of waves of greater or lesser length. Nervous derangement and prolonged excitation are, then, causes which may produce temporary green colour-blindness.—Silliman's *American Journal*, Jan. 1877.

ON SOME EFFECTS OF HEAT UPON VOLTAIC CIRCUITS COMPLETED BY AN ELECTROLYTE. BY W. HELLESEN OF COPENHAGEN.

I. It is known, and M. du Moncel has demonstrated in several ways[*], that if one of the plates of an electrolyte be heated, a thermoelectric action is produced, which has the effect of producing a current, the heated plate constituting the positive pole. This action, which often shows itself in aerial telegraphic circuits[†], may become very energetic if the electrolytic system be disposed so as to maintain the liquid around the two plates at considerably different temperatures ; and to obtain this result, we need only isolate from one another the portions of the liquid into which the electrodes dip—or to take advantage of the property possessed by all liquids, of keeping at their surface the most heated portions, which thus, in some measure, float upon the cool portions. To effect this disposition, the electrolytic system may be constituted by two testtubes of glass, joined near their apertures by a tube of some centims. length, and having fixed in them the two electrodes, one at the top, and the other at the bottom. In this way, when the apparatus is filled with liquid, one of the electrodes dips into the part near the surface of the liquid, the other into the opposite part ; and the liquid being confined in two receivers communicating only through the tube which joins them, may present in each test-tube a very different temperature. In these conditions, if we take for the liquid a saturated solution of sulphate of copper, and copper plates for electrodes, on heating with a spirit-lamp the test-tube of which the electrode occupies the upper part, we obtain a relatively energetic current, which can be ascertained with not very sensitive rheometers ; and the electrode, thus heated through the intermediation of the liquid, becomes promptly covered with a deposit of copper.

The experiment is also successful on employing as electrodes platinum and lead, and water acidulated with sulphuric acid as the electrolytic conductor. The above arrangement, however, with metallic electrodes that are good conductors of heat, such as plates of copper, and a solution of sulphate of copper, may be considerably simplified. It then suffices to fix on the brim of the vessel filled with this solution one of the copper plates, which must be bent so as to touch with one end the surface of the liquid, and to present at the other, outside the vessel, a sufficiently large surface to be heated. On immersing the other plate to the bottom of the vessel and uniting it to the first by a wire covered with gutta percha or caoutchouc, we obtain by heating this first plate a current almost as powerful as with the other arrangement.

[*] See *Comptes Rendus*, 1872, t. lxxv. pp. 958, 1100, 1504.
[†] Ibid. pp. 958, 1623.

The energetic effect determined by both these arrangements evidently depends on the circumstance that the heated portions of the liquid tend to maintain themselves at its surface, leaving the lower parts at a less-elevated temperature; and if Faraday's experiment in a U-tube did not give any decisive result, it was because, in consequence of the disposition of the apparatus, the liquid about the electrodes did not present a sufficiently great difference of temperature.

By making use of magnetic helices, of which the wire would be constituted by copper tubes in which currents of cold water and of superheated steam circulate, it might perhaps be possible to obtain, in consequence of the preceding reactions, magnetic effects which would explain the magnetic state of the terrestrial globe by attributing it to the inequality of temperature of the seas.

II. If the electrolyte be constituted by the smoky flame of a paraffine taper, and if the current from a pile of 40 Leclanché elements, or of 30 elements with bichromate of potash, be passed through the flame, a copious arborescent deposit of carbon is observed to form at first at the extremity of the negative wire, constituting an agglomeration of branches tending to rise in the direction of the flame, and to reach the positive wire. A few moments after, carbonaceous threads, slightly curved, are deposited upon this, and likewise tend to rise with the flame; and if these two deposits be brought near to one another by bending their ramifications a little, a series of bright sparks are seen to be exchanged between the extremities of the ramifications, resembling points of electric light.

III. When for the short wire which connects the anvil of the interrupter of a Ruhmkorff coil with its condenser a long wire is substituted wound parallel with another wire upon a second coil, at the instant the extremities of the second insulated wire are placed a short distance from one another, a pretty large spark is observed, which can only proceed from the discharge of the condenser; for it can be produced with short wires not wound in a spiral. It is not affected by a magnetic core placed in the second coil; and the inducing current of the induction-apparatus does not pass into the second coil. On the other hand, it is augmented with the size of the condenser, and depends on the nature of its insulating plates: the most energetic effects are obtained when these plates are of mica. This spark has acquired sufficient tension to traverse a stratum of air of some millimetres, even when to produce it only a single Bunsen element of the medium model, a coil of average size, and a secondary wire of 10 metres were employed. —*Comptes Rendus de l'Académie des Sciences,* Jan. 8, 1877, pp. 83–85.

DR. J. CHRISTIAN POGGENDORFF.

It is with sincere regret we have to announce the death of our old and highly esteemed friend Professor Poggendorff, which took place on the 24th ultimo, a few days after the completion of his 80th year.—W. F.

XXIV. *On the Laws of the Passage of Electricity through Gases. By* G. WIEDEMANN*.

SOME time since the author, in conjunction with Dr. Rühlmann, made a series of experiments on the discharge of electricity between spheres of metal in rarefied air. The image of the discharge was reflected from a revolving mirror fixed on the axis of a Holtz machine which supplied the electricity. The reflected rays passed through a telescope, the object-glass of which was divided vertically, like a heliometer, into two parts, one of which was displaced until the image of one discharge seen in one half was brought into a line with that of the next discharge seen in the other half. The amount of displacement was measured.

In all the experiments the pressure of the gas was so far reduced that the discharge took place through the gas alone, unmixed with any material particles from the electrodes.

These experiments led to the following conclusions :—

I. *That the potential on the spherical electrodes, necessary for effecting a discharge, is greater for the positive than for the negative electrode.*

This result was obtained by measuring the time-interval between two consecutive discharges—(1) when one electrode was charged with positive or negative electricity and the other connected with the earth, (2) when both electrodes were connected with the machine, but one was larger than the other.

II. *The quantity of electricity necessary fo reffecting a dis-*

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charge increases with the pressure, at first rapidly, then more slowly and nearly in proportion to the increase of pressure. These quantities of electricity are, under circumstances otherwise similar, greater and greater for the following gases in order, hydrogen, oxygen, carbonic acid, air, and nitrogen.

From the difference of charge on the two electrodes necessary for effecting a discharge, an explanation of the various features of the discharge was deduced.

On account of their greater potential, the gas-molecules at the positive electrode are repelled with a greater velocity than those at the negative ; and consequently the positive discharge extends to a greater distance from the positive electrode than the negative from the negative. Further, since the difference of tension at different points of an electrode is greater with large than with small charges, the discharge from the positive takes place only from the point directed towards the negative, while that from the negative takes place more largely from the general surface. When the discharge takes place between a plate and a small sphere, we find at the meeting of the two discharges phenomena similar to those produced at the meeting of two currents of water. If the plate is negative, it exhibits the glow or halo, while the positive discharge from the sphere sinks in a cup-shape ; if the plate be positive, the red positive discharge from it is raised towards the halo on the sphere.

Similarly we may find an explanation of the action of the electric egg, and of the connexion between the direction and velocity of the magnetic rotation of a discharge in rarefied air (between a point and a ring) and the direction of the discharge.

In the later experiments one of the spherical electrodes of the vacuum-tube was connected with one conductor of the Holtz machine, the other conductor of which was connected with the earth, while the second electrode was connected with a galvanometer and thence with the earth. Whether the first electrode was charged with positive or with negative electricity, the alternating discharges, and consequently the quantity of electricity passing through the tube, remained unchanged. But still the number of discharges in a unit of time was greater when the electricity drawn from the machine was negative.

The above-mentioned difference of quantity of positive and negative electricity necessary for effecting a discharge is not determined by the passage of unequal quantities through the apparatus used for the discharge. The same result will be obtained if a spectrum-tube, *i. e.* a capillary tube with spherical

ends, be used.   An examination of the number of discharges through gas at different pressures led to the following conclusion :—

III. *The quantity of electricity necessary for effecting a discharge in a spectrum-tube increases with the pressure, at first rapidly, then more slowly until the pressure reaches* 20 *to* 30 *millims.   Beyond this the quantity of electricity increases again more rapidly, and becomes nearly proportional to the pressure.*

The reason for these results, which differ from those obtained in the case of discharges between spheres in a free space, is to be found in the charge on the glass envelope.   With low pressures the density of this is small, and diminishes only slightly towards the capillary part of the tube, and the discharge consequently proceeds from a larger part of the surface of the electrodes.   With higher pressures the density of the charge on the glass, and its diminution towards the capillary part, are greater ; the discharge proceeds more from a small surface near the ends of the electrodes ; *cæteris paribus,* a smaller quantity of electricity suffices for the potential necessary to effect a discharge, while on the other hand the higher pressure requires a larger quantity.   The curve which represents the relation between the quantity of electricity and the pressure becomes more and more parallel to the line of abscissæ.   The more the discharge is confined to the point of the electrode, the more rapidly does the quantity of electricity increase with the pressure.

By varying the length of the capillary part of the tube, the author was led to the following conclusion :—

IV. *The quantity of electricity necessary to effect a discharge is, within certain limits, independent of the length or breadth of the tube joining the chambers which surround the electrodes.*

If these chambers are spherical, like the electrodes themselves, the quantity of electricity is not perceptibly dependent upon the radius of the spheres, as may easily be concluded from the absence of action of the electricity collected in the glass chambers upon electrical points in their interior.   Only in the case of small glass spheres (in which the electricity distributed on the capillary part, on account of its less density and smaller distance from the electrodes, obtains an influence) is the quantity of electricity necessary for a discharge less. Also, in spectrum-tubes the necessary quantity of electricity is less with hydrogen than with air, &c.   From this, if we neglect secondary circumstances, we may conclude :—

V. *With a given gas at a given pressure the quantity of electricity necessary to effect a discharge depends only on the environment of the electrodes.*

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While the foregoing researches lead us to some conclusions respecting the quantity of electricity necessary for the *beginning* of a discharge, it is requisite through a measurement of the heat developed, to learn something definite about the progress of the discharge *during the passage of the electricity.*   By application of a thermopile to the different sections of the capillary tube, the rise of temperature due to the quantity of heat developed in a unit of time may be determined.   These quantities of heat in various sections of a capillary tube are the same whether the tube be long or short, even when the length is varied in the proportion $1 : 1 \cdot 78$; only in longer tubes the heating, and with it the brilliancy of the discharge, diminishes towards the electrode connected with earth, especially when it is negative.

VI. *Tubes of various internal, but equal external diameter receive, under circumstances otherwise similar, nearly equal quantities of heat in equal times from the discharge,* even when the cross section of the space through which the discharge passes is increased in the proportion $1 : 4.$ This condition of things is maintained when capillary tubes of different diameters, or when tubes of diameters varying at different parts, are used.

If the capillary tube is slightly heated at any part, the number of discharges scarcely alters.   If it be considerably heated, a bright sodium light appears at the heating-point, and the number of discharges increases.   But the sodium light does not extend beyond the heated part.   If the space about the electrodes be heated, a sodium light is similarly developed without spreading further ; the intervals of the discharges increase.   But as soon as the pressure due to the heating reverts to what it was before, the intervals resume their former value, even though the glass be heated almost to fusion.

The charge on a capillary tube, examined electroscopically throughout its entire length, is similar to that of the electrode connected with the machine, provided that the other electrode be put to earth.   It diminishes gradually towards the latter electrode.   If both electrodes are connected with the machine, the charges on the capillary tube diminish from each end to a neutral point in the middle.

With a view of obtaining some information about the nature of the dark space near the negative electrode, some experiments were made upon the effect on the discharge due to an interruption of the circuit by a slight interval of air.   For this purpose a cylindrical tube was used.   Its length was 30 centims., and breadth 30 millims. ; it contained rarefied

air at a pressure of from 0·5 to 1 millim. ; and it was furnished at both ends with platinum electrodes having their axes perpendicular to that of the tube. When the tube was insulated, and both terminals directly connected with the conductors of a Holtz machine, both electrodes were covered with a bright halo. From the positive there proceeded a somewhat contracting discharge, A, which at some distance from the electrode showed a dark spot, B, and then slightly decreasing (C) through the entire tube, passed to the neighbourhood of the negative electrode, from which it was separated by the dark space D.

On putting the positive electrode to earth, the dark spot B becomes smaller, while the discharge A spreads somewhat further. On putting the negative to earth the dark space D becomes larger, and the part C of the discharge contracts in a spindle-shape towards it.

A small air-spark about 1 millim. in length was then introduced in the conducting wire of one of the electrodes. If the spark was at the negative electrode, the dark space D appeared much more sharply defined and more broadly developed, while from the small dark spot B close to the positive electrode the red luminous discharge C, at first somewhat diminishing and then again increasing, extended through the tube to D.

If the positive electrode was put to earth, the dark spot B disappeared almost entirely ; otherwise the appearance remained nearly unchanged. If the spark was increased, the dark space D became larger ; and when it was considerably increased, a bright streak extended from the lowest point of the electrode to the other luminous discharge. If the current flowing to the negative electrode was retarded by a column of water, the extent of the dark space D diminished more and more.

If the negative electricity of the machine was put to earth before the air-spark, the dark spot B at the positive electrode became smaller, the succeeding part of the discharge C sharpened to a point towards the centre of the tube where the contraction had taken place shortly before, and at the same time it increased in brightness. But before coming to a point it passed into a feebler column of light, which filled the tube, and was separated from the negative electrode by the very sharply and broadly marked space D.

When the spark was introduced at the positive electrode, that electrode appeared covered with a brighter halo than the negative. From the former there proceeded a column of grey light, which filled the whole tube ; this slightly contracted conically towards the negative electrode, and became brighter

towards the vertex, but it extended to the negative electrode
without being divided from it by any dark space.

On putting the negative electrode to earth, the appearance
was but slightly altered; only the bright part was advanced a
little, and the discharge was wider. On putting the positive
electricity of the machine to earth, another, darker spot ap-
peared at the positive electrode; the dark space at the nega-
tive electrode was hardly perceptible. With a very small air-
spark and somewhat higher pressure of gas the discharge merely
passed between the ends of the electrodes in an arc, and was
scarcely altered by the interposition of a column of water.
The charging of the sides of the tube with free electricity is
seen by the following Table :—

Electrode.	Circuit unbroken.	Air-spark at the + electrode.	Air-spark at the − electrode.
Insulated	Whole tube −.	Whole tube − diminishing from − electrode.	Whole tube + diminishing from + electrode.
+ put to earth...	Diminishing +.	Greatly diminishing −.	Weak − at the − electrode, otherwise almost unelectric.
− put to earth...	Diminishing −.	Weak + at the + electrode, otherwise almost unelectric.	Greatly diminishing +.

With regard to the passage of electricity through gases, two
opinions have been put forward. On the one hand it was
supposed (and the opinion was suggested as the more probable
in the former memoir on the subject) that the particles of gas
situated at the electrodes carry with them the electricity at
the commencement of the discharge, and transmit the dis-
charge from one electrode to the other by means of a directly
progressive movement of the particles. Against this view
various physicists, and among them A. de la Rive, maintain
" that in gases of a certain density the discharge is indeed
disruptive, and therefore that gas, like every insulating body,
whether solid or fluid, plays the essentially passive part of a
hindrance to the re-connexion of the two electrodes, which, so
to speak, is mechanically overcome by the discharge. On the
other hand, gas of a certain density or rarefaction will allow
the electricity to flow out without mechanical resistance, and
will only offer an electrical resistance, while the transmission
of electricity takes place by so rapid a sequence of molecular

movements that it appears almost continuous. We then have a real galvanic current, upon which the magnet acts as upon a rigid conductor, and which is capable of effecting electrolytic decomposition of the gas, if it is compound, as in a liquid electrolyte. In this case the resistance will be in proportion to the length of the column of gas. It is therefore no longer a discharge, but a real electric current." In proof of this view it is especially urged that when induction-currents are employed, besides the disruptive discharge, the "aureole" appears, as well as the "trait de feu," which disappears in highly rarefied gases, and, according to Perrot's experiments, these can be separated from one another.

But if an extremily rarefied gas acted in the same way as a metallic conductor or an electrolyte, the smallest difference of tension at the electrodes would cause a proportionate current to pass through the gas, and the electricity could not, as experience shows, accumulate before the commencement of the discharge in a greater but finite density.

To make it possible to combine the latter phenomenon with the supposition of an electrolytic conduction of the gas, we must form the hypothesis of a certain resistance at the electrode itself, which, like a non-conductor, would prevent the passage of electricity from the electrodes to the conducting gas, and to break through which a finite tension would be requisite.

The resistance itself could only have its origin in the stratum of gas condensed on the surface of the electrodes, the density of which is partly due to external pressure. But the experiments upon galvanic polarization by gas at diminishing pressures show that the density of a gas stratum condensed upon metal is very little influenced by the pressure ; while on the other hand the experiments upon the electric discharge show that the mass of electricity necessary for the production of a discharge, and therefore the supposed resistance, increase with the pressure. If the electrodes be heated to about the temperature of melting glass, the gas stratum should also change ; but the quantity of electricity remains remarkably constant if the pressure be unchanged. Furthermore, it would follow from these experiments that the resistance is greater at the positive than at the negative electrode, while yet we cannot perceive why greater power should be required at the negative than at the positive to break through the stratum of condensed gas. To explain these different conditions at the two electrodes, we should have, beside the supposition of resistance, to form a second hypothesis, somewhat like that mentioned in the former memoir, that the

metals attract positive more strongly than negative electricity, and therefore that, if charged with an equal quantity of electricity, the negative electrode obtains upon its surface the tension necessary for the commencement of a discharge sooner than the positive. After the resistance has been overcome, the column of gas between the electrodes should, on the hypothesis now under discussion, conduct the electricity like a metal. But we find that, with similar electrodes and similar gas at various degrees of pressure, the quantity of electricity requisite for a discharge is at first independent of the length of the distance between the electrodes—as for instance in capillary tubes when their length is so great that the influence of the electrodes upon each other and that of the free electricity accumulated in the tube may be neglected. Again, in capillary tubes of different lengths the distribution of electricity upon the charged electrodes before the discharge must be equal, and therefore also the resistance that has to be overcome at the electrodes must be similarly equal. In fact, if we put v for the potential energy which corresponds to the electrical charge immediately before the discharge, a for the action effected in overcoming the resistance, b for the action effected in the " metallically conducting gas " upon a unit of length in the shape of production of heat, and l for the distance between the electrodes (the wider spaces in the neighbourhood of the electrodes being replaced by a suitable length of capillary tube), the result will be

$$v = a + bl, \text{ or } b = (v - a) : l.$$

Thus the action effected, or the heat produced in the entire length of the capillary tube would be inversely proportional to the length of the tube. But experiments show that the quantity of heat is, within certain limits at least, nearly independent of the length of the tube.

We find a similar contradiction if we measure the heat produced by discharges under conditions altogether similar, except the diameter of the tubes.

If the gas be heated like a metallic conductor, we must suppose that the whole heat produced in the gas by the discharge passes over to the capillary tubes. Accordingly the increase of temperature in a tube of diameter 4 would be only one fourth of that in one of a diameter 1, if the gas in both tubes were equally good as metallic conductors. But as a matter of fact the increase of temperature is in both cases almost the same. If we suppose the conductive power of the gas to be itself dependent upon its temperature, we must first explain, since the gas after each single discharge becomes

perfectly dark and therefore cold, in what manner the gas conveys the electricity before becoming heated to the temperature at which it begins to conduct like a metal, and how it thus becomes heated. Besides, the increase of temperature in tubes of different diameters must be precisely such at each pressure, that, in addition to the heat formerly produced in them, so much more heat should be produced in the gases which now conduct metallically that the combined bodies of heat in the tubes would be equal to each other—a supposition equally complicated and improbable.

Even the phenomenon of the aureole and the "trait de feu" is no proof of the metallic conductivity of the gas, as the former is equally produced by single discharges in luminous gases, and the latter by metallic discharges which at high pressure are added to the former.

Rather do the above-mentioned experiments allow us to conclude almost with certainty that the conveyance in gases is not carried on by any such double process, but actually continues essentially in the same manner from the beginning to the end of the discharge.

The process of the discharge in my opinion may be supposed to be somewhat as follows. First of all, we cannot suppose that the conveyance of electricity in gases can take place through the ether alone ; for then a discharge would pass in all vacua, however perfect, which is not the case. The particles of the gas themselves must therefore take part in the conveyance of electricity. If, then, the charge of the electrodes at one place exceeds a certain limit, and if the scattered particles of gas lying nearest to the electrode, between it and the other parts of the gas, be also sufficiently strongly charged with electricity of the same name, the whole of the electrified mass of gas there will be thrown up, as when a liquid is boiled. But even when the electrodes are considerably heated the number of discharges with an equal supply of electricity does not alter, provided the pressure remains constant. Hence the overcoming the attraction of the gas stratum condensed upon the electrodes, which is certainly altered, plays only a subordinate part in the phenomenon.

The body of gas electrified at the electrode moves away in the direction in which the tension most rapidly diminishes, with a velocity proportional to the force which immediately before was pressing the gas particles upon the electrodes.

Here the *original* impulse occurs only upon the electrodes ; and in the case of a pair placed opposite to one another in a free gas space, it will depend only upon the tension existing upon particular points of them. If the electrodes are en-

closed in tubes, this original impulse, and the quantity of electricity necessary for a discharge, are on this very account independent of the length and width of the tube—in so far at least as the mutual action of the electricities accumulated upon the electrodes, and that of the free electricity distributed along the tube upon the electrodes themselves, can be neglected.

In like manner the tension required for a discharge is independent of the heating of the gas in the capillary tube, so long as the glass is not decomposed by the heat and transformed into a metallic conductor, because such a transformation would be equivalent to interposing a new electrode between the original electrodes of the tube.

The tension required for the commencement of a discharge in different gases may depend, under conditions otherwise similar, upon the nature of the gas itself, upon the capacity for a charge of its several molecules, &c.

In a gas-discharge proper the molecules do not fly from one electrode to another ; against such an idea there may be cited both the kinetic theory of gases, and the fact that sodium vapour rendered luminous, either at an electrode or in any part of a capillary tube, by a discharge passing through it, remains nearly in the same place, instead of spreading itself at once in smaller or greater quantity through the whole tube.   When also at higher pressure and with greater charges, metallic discharges appear beside the gas-discharges between two electrodes, whether in a free gas space or in a tube, there are formed at the electrodes small tufts of luminous metallic particles, visible by the spectroscope.   The higher the pressure, the further these extend towards the opposite electrode ; but only at a comparatively high pressure do they span the entire interval.

According to this, the passage of electricity between the gas particles in a free space ought to take place at first exactly as in a row of elastic balls hanging side by side. When the charged gas-molecules A escaping from the electrodes meet the more distant B, they separate the electricities of the latter, and neutralize a part of their own electricity with that of the opposite name of B, so that both molecules appear to be charged with electricity of the same name.   The recoil resulting from this drives A back and B forward.   A receives fresh electricity either from the newly advancing molecules or from the electrode, and again moves forward ; while B, driven on by the electric recoil and the movement imparted to it by A, gives up a part of its electricity to a third portion, C, and falling back, again joins A, and so on.

The similar movements proceeding from the two electrodes meet in one part of the tube, where the balancing of the opposed electricities of the now mutually attractive last molecules takes place. During this process the velocity of the single gas-molecules may become so great as to render the gas luminous. Meanwhile the action effected by the union of the opposed electricities in two consecutive molecules will be compensated by the division of the electricities in each molecule.

If the gas be capable of electrolytic decomposition, the movements of the gas-molecules, as above described, may be connected with a separation, dispersion, and reunion of their atoms, similar to other electrolytic processes, as shown by the experiments of Perrot*.

If the electrodes in a glass tube be so far apart that their mutual influence may be neglected, the phenomena become more complicated, because the sides of the tube may become charged from the electrodes. Although with the apparatus above described the so-called "positive discharge" always passes through the entire length of the capillary tube, it is only when the negative electrode is put to earth that the whole tube receives a positive charge, diminishing of course in strength with the distance from the positive electrode.

With insulated electrodes it is only in part positively charged; but when the positive electrode is put to earth it is negatively charged from the negative electrode in its entire length, with diminishing intensity. This free electricity, as we know, holds the opposing electricity on the outer surface of the glass, while that of the same name becomes more or less dispersed. The electricity distributed upon the inner side of the glass tube may have some effect upon the tension at the electrodes. It has also some influence upon the velocity and direction of the movement of the gas-molecules, as is very clearly shown by experiments with the larger glass tube. The principal impulse, however, appears, as in the free space, to proceed from the electrodes themselves; for instance, if the positive electrode be put to earth, the fall of electricity measured from the negative electrode in a tube of the length 2 is indeed only half as great as in a tube of length 1; but yet the production of heat per unit of length in both tubes is nearly equal.

The transmission of motion from gas molecule to gas molecule occasioned by the partition of electricity in the tubes should, if we neglect the communication of heat to the sides, take place in accordance with the laws of elastic impact. If in this case the discharge proceeds from a wider

* Cf. *Galvanismus*, 2. Aufl. Bd. ii. S. 412.

to a narrower tube, in whose cross section the number of gas molecules is equal to *a*, the combined total *vis viva* in each section still remains the same, because during the same time in both tubes a molecule set in motion meets with an equal number of comparatively quiescent molecules, although the oblique angle at which the gas particles first electrically moved meet others causes the movement to spread, in a direction partly radiating, partly axial, over the entire section.

This simple transference of motion is accompanied by a partial conveyance of it to the sides of the tube, by which the *vis viva* of the gas-molecules, and therefore also the temperature of the gas, diminishes from the part of the tube nearest to the place of discharge to the furthest. As, however, the lateral spread of the *vis viva*, in consequence of the excentric impact of the molecules upon each other both in narrow and in wide tubes, whether through more or less concentric strata of molecules, proceeds in the same way to the periphery, it follows that the communication of movement, as well as of heat, to the sides, which is in proportion to the number of the molecules that strike them, must be the same in wider and in narrower tubes.

As in most experiments this communication of heat is but small, the points furthest from the place of exit of the discharge in the shorter tubes are but little cooler than the nearest points, although in longer tubes the cooling is very plainly perceptible by the lesser brightness of the discharge.

That the passage from the brightly luminous parts of the tube to the darker in such a case follows rather quickly, must be caused by the fact that the brightness of the glow of different bodies, as, for instance, luminous galvanic wires, increases very rapidly with the temperature.

The opinions stated above agree substantially with our present views upon the constitution of bodies and the conduction of electricity through them.

I must ask permission here to touch a little more closely upon this subject, as some hypotheses lately put forward concerning it appear to me not entirely to agree with experience.

In solid bodies the molecules vibrate round fixed centres of equilibrium, which are not substantially altered when, in the current, electricity passes from one molecule to another. According to one view this passage occurs by *single disruptive discharges*. We must then suppose that in some way the molecules of these bodies are united with coverings of particles of electricity, which share more or less in the oscillatory

movements of the molecules, and that particles of electricity pass from one covering to another. Electric vibrations might also be communicated from one molecule to another. The existence of a resistance to conduction is sought by Herwig in the fact that in general the direction of movement of the particles of bodies with their coverings in two successive sections of the conductor is very different. In the passage, therefore, of electric particles from one molecule to another the direction of their movements must alter ; and the work hereby done would correspond to the resistance, which would be all the greater the greater was the energy of the movement of the particles of the bodies, or the higher the temperature.

In this case, however, no actual *change* in the *direction* of movement takes place, but only a combination of two differently directed movements. If we resolve the entire movements in three rectangular directions, on the passage of one of these components from one molecule to another its *vis viva* is merely added to that of the components perpendicular to it, as in the combination of two perpendicular vibrations of light. On the other hand, there may take place eventually a change in the *vis viva* when two *similarly* directed components meet together. But this alteration will in the mean be compensated, since in a constant current as much electricity enters a molecule as leaves it.

If, then, we suppose a passage of electricity or of electric vibrational movement from one molecule to another, the perpetual impulse to motion, consequent on the electromotive force in a constant current, must be compensated by a kind of friction of the electric particles on the molecules of bodies, by which the former will be entirely consumed. This frictional resistance corresponds, under an equal electromotive force, to the galvanic resistance to conduction. By this means the *vis viva* of the electric motion passes to the particles of bodies ; the *vis viva* of their heat-vibrations increases in proportion to the resistance which they oppose to the current *, &c.

In consequence of the comparative ease with which the molecules of bodies move at higher temperatures, especially above the fusing-point, when their relative positions are less rigidly maintained by the molecular forces, the communication of the movement of electricity to the single molecules of bodies would then more easily ensue, and the resistance would therefore increase.

In fused and fluid metallic conductors meanwhile a part of the electric conduction might result from the conveyance of

* Compare on this point *Galvanismus* (2), Bd. ii. S. 1168.

electricity with the molecules proceeding in streams from their centres of equilibrium, which would then communicate it to the succeeding molecules, and so on.

In all probability electrolytes are conductors in consequence of the electricity being carried by the atoms with which it is connected.   Whilst the atoms of each molecule in their movement come into friction with each other, with the dissolving medium, &c., they lose their electrolytic movement in the direction of the current, and their *vis viva* is converted into heat-motion.   This frictional resistance therefore answers to the resistance to conduction.   On account of its complexity, especially when the electrolyte is in solution, we cannot expect to find a simple relation between the resistance of the electrolyte and its otherwise mechanical constants.   On this account also, a simple law cannot be expected, for instance, from the comparison of the resistance of the fluid solutions of chlorides with their specific volumes.   The conductive capacity calculated upon the supposition of an inverse proportion between two values differs, for instance. according to F. Kohlrausch, nearly 10 per cent. in quantity.

The gas molecules then, as above stated, will be propelled from the electrodes with a certain charge in directions which correspond to the greatest diminution of the potential ; they thus come into contact with other molecules, with which they share their electricity or (if electricity consists in oscillations of the ether surrounding each molecule) their vibrations, and so on.

The process, therefore, would be exactly similar to that of electrolysis, only that the *vis viva* of the gas molecules does not, like that of the atoms, so far as produced by the electromotive force acting at each point, change completely into heat by friction on the neighbouring medium, but it propagates itself from the electrodes through the whole interval of the discharge, and a small aliquot part of it is also conveyed about equally all along the sides of the tube in the form of heat.

Besides the general characteristics of discharge, there is another series of separate points to be noticed—for example, the unequal extension of the positive and negative discharge, the spot at which the *vis viva* contained in the moving bodies of gas ultimately appears in the form of heat, the dark space near the negative electrode, and the disposition of the light of the discharge.

The unequal extension of the positive and negative discharge has already been referred, in a former memoir, to the greater initial velocity in the positive as compared with the negative

discharge, consequent upon the greater charge which has been proved by experiment to be necessary to the existence of the former.

In explanation of this circumstance the hypothesis was there advanced of the unequal attraction of the two electricities by the electrodes, which, at least so long as we are obliged to retain our present conceptions of the nature of electricity, agrees with the theory which we have formed of the electromotive action of two metals on one another.

As in my former memoir I did not dwell on the explanation given by other physicists of these phenomena and of others nearly connected with them, I will venture to touch upon them again briefly here.

In his very interesting experiments on Lichtenberg's figures, Herr von Bezold * advances the hypothesis, that when they are exhibited in the positive terminal a movement takes place towards the conductor, when at the negative a movement towards the periphery. The same circumstance does not seem to occur in gas-discharges. If with increased pressure tuft-shaped metal-discharges accompany the gas-discharge at the positive electrode, the glowing portions of the metal must separate themselves from the electrode and move away from it.

In the same way, in Geissler tubes a gradual conveyance of the metal from the negative electrode to the surrounding glass will be observed. Both phenomena, however, point to a continuous movement from the electrodes of molecules participating in the discharge, and cannot well be explained by any kind of absorption at the positive electrode.

According to P. Riess †, the difference between the positive and negative discharge would be much more easily realized if we supposed that the current of air which always accompanies the spark of the electrode and passes by the moistened electrode, by some kind of friction with the dry air makes the latter negatively electric.

According to the analogy of Lulliu's experiments and electric dust figures, the positive electricity would subsequently be able to spread out wider in the negative space. Setting aside the fact that the differences of the gas-discharges at the two electrodes appear also in the most carefully dried space, the positive electrode would, contrary to the result of experiment, obviously require for the discharge, according to this hypothesis, a lesser tension than the negative, since the negative electricity of the air already exercises an influence on the positive electricity of the electrode. Whether under the

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* Cf. *Galvanismus*, Bd. ii. S. 936.
† *Reibungselectricität*, Bd. ii. S. 751.

same influence the positive discharge would spread out wider than the negative in the negative air, in which it is already partially neutralized, must be a question for a special investigation.

In another place P. Riess* attempts to derive the action of the electric egg from the fact that the air which surrounds the electrode is charged with the electricity of the electrode by the glow discharge which always precedes the explosive discharge, and this air must be broken through subsequently by the explosive discharge ; therefore a discharge in a direction opposite to the first would be more readily conveyed between the electrodes than one in the same direction.

Since the glow discharge in rarefied air on a large surface takes place most readily when the surface is negative, so in the alternating discharge of the electric egg the large electrode will more readily show a glow when it is negative, and will therefore more easily permit the passage of the current, for which it serves as a positive electrode. The last phenomenon of discharge may therefore be referred to a previous contrary current, and a consequent secondary charge of the air. As, however, the passage of electricity between a large and a small electrode in a space of rarefied air follows precisely the same conditions, even when no contrary current has previously passed between the electrodes, the above hypothesis cannot be absolutely correct. The fact that a large negative electrode becomes luminous in rarefied air more readily than a positive one, also requires explanation. The experiments mentioned in this and the former memoir ought to account for this, as well as for the characteristics of the electric egg, even without the supposition of secondary occurrences. If a series of discharges, having passed for a length of time in a particular direction through a tube, is suddenly reversed, the discharges then first ensuing undoubtedly take place somewhat quicker than those which follow—because the glass is still charged in a manner corresponding to the direction of the first current, and accelerates the speed of the opposite electricities conveyed to the electrodes by the reversal of the current. Soon, however, the normal charge of the glass is restored, and the succession of discharges is perfectly regular.

The question further arises, at what point the electric movements of the gas molecules which advance with different velocities from the two electrodes meet and the opposite electricities at length neutralize one another.

* *Berliner Monatsber.*, 6. August 1868. Pogg. *Annalen*, Bd. cxxxvi. S. 44. Cf. *Galvanismus* (2), Bd. ii. S. 941.

With reference to this question we must recur to the experiments in which one electrode is put to earth while the electricity of the machine is transmitted to the other through an air-spark. In this case the air-charge must begin at once, on the passage of the air-spark, with a certain accumulation of electricity, and without any previous distribution of static electricity upon the sides of the glass ; in conformity with this we find here no distinct charge of the sides of the tube.

When the discharge proceeding from the charged electrode passes through the tube, the two electricities of the electrode which is put to earth are also divided, and from the latter also the discharge begins, though only a short time after the other. Both discharges (which, though the movement proceeds from a communication between the molecules, still produce the impression of currents of air) meet towards the electrode which is put to earth. Being thus attracted to one another, they contract ; and since the discharge proceeding from the positive electrode begins with greater velocity than the negative, it penetrates more or less into the latter and presents to it a convex surface ; and since at the meeting-point the opposing electric molecules are much accelerated, the discharge there appears brighter.

As the dark space appears near the negative electrode, even when the electricity is conveyed to it through an air-spark and when the positive electrode is put to earth, it cannot be the meeting-point of the positive and negative discharge, or the place where the free electricity of the two are neutralized, since this would rather be warmer and brighter, as mentioned above. As, however, the dark space, according to De la Rive's direct experiments, appears colder than the other part of the discharge, and a true current of electricity cannot certainly be proved to exist in it by means of derived currents (by which new electrodes are brought into the current), it therefore follows that the passage of electricity through the dark space must take place in a manner different from that of the rest of the discharge. Its appearance must depend upon the magnitude of the charge on the electrodes necessary for the commencement of a discharge, because under circumstances otherwise similar it appears, to a certain extent, only when the positive electrode is put to earth and the air-spark is introduced between the machine and the negative electrode, and not in a reversed arrangement.

According to the experiments mentioned at the beginning of this paper, the extent of the dark space increases with the length of air-spark, it diminishes when between it and the

negative electrode a water resistance is interposed ; its extent increases therefore with the quantity of electricity conducted to the electrode in a given time ; meanwhile, except with very great quantities of electricity, the negative electrode is illuminated throughout. At the positive electrode the same influences only produce a greater or smaller extension of the discharge on the electrode. It is smaller in proportion to the supply of electricity.

According to the opinion expressed above, it might be thought that, starting from the negative electrode (supposed to be directly connected with the source of electricity), not only the gas molecules which are in immediate contact with it, but also those that are at a distance, receive a distribution of electricity diminishing with the distance. If the electrode is opposite to a glass wall which is eventually put to earth externally, this direct induction from the electrode extends yet further. With an increasing charge the portions of gas approach nearer to the electrode, so far as the elastic properties of the gas permit. In front of them appears, for the moment, a rarefied space. If at the commencement of the discharge the negatively charged portions of gas which touch the electrode are driven away, they impart their charge to the following set of molecules ; then either they, or other portions of gas in succession, charged with divided electricity betake themselves to the electrode, where they neutralize the electricity of the opposite name and become negatively charged. In consequence of this, not only is the electrode much heated, but a great velocity is imparted to the particles. Wherever this communication between the electricities takes place, whether between the electrode and the particles, or between the particles themselves, the blue halo must appear and eventually spread itself as far as the opposite glass wall. The heating of the electrode is therefore immediately connected with its appearance. This sort of discharge will last so long as the charge of the electrode is powerful enough to impart to the particles a velocity corresponding to the pressure.

If the negatively charged electric particles in succession leave the neighbourhood of the electrode and traverse the rarefied space in front of them, there ensue few, if any, encounters with other molecules ; on the contrary, the particles, in consequence of their mutual electric repulsion, occupy a larger space, and their temperature and brightness appear, therefore, less than where they are pressed close to the electrode ; so that the space will appear either dark, or illuminated only by a dull and cloudy light. Only when the negatively charged molecules strike the uncharged, and charge

them, does a close aggregation ensue; the heat and brightness then become greater, and are transmitted, as stated above, from particle to particle. Possibly, after the first collision, a large number of consecutive equally charged molecules may travel some distance without a fresh collision between each other, in the same way as the molecules which first left the negative electrode; and they may only then transmit their charge to the unelectric molecules beyond them. In this way lighter and darker patches may appear alternately, forming the well-known striæ.

At the positive electrode the discharge begins at a much higher charge than at the negative. With the same supply of electricity it will pass in a much shorter time. The gas molecules thrown off from it will be fewer; but they will be more highly charged, and will move from it with great velocity. The dark space, therefore, disappears. Only in particular cases does it exist to a smaller extent, viz. when the electrodes are directly connected with the machine, and when an air-spark is introduced at the negative electrode, by which means a reverse discharge of the tube, which has been charged by the positive electrode, may commence through the latter before the spark can break through the interval at the negative electrode.

I do not deny that many experiments will be needed to prove these theories. But they essentially comprise the phenomena under one general aspect, which involves no suppositions beyond those involved in our previous theories of the nature of electricity. The great difficulty and complication of the phenomena may make this preliminary statement not wholly needless; and I hope to be able to supply some further contributions hereafter.

We ought now to examine whether and how far the above mentioned researches, and the differences deduced from them as to the behaviour of the discharges at the positive and negative electrodes (which for brevity we may generally term positive and negative discharges), will explain the very interesting and remarkable details of their outward appearance, noticed more particularly by Hittorf[*], with electrodes of different forms. We must necessarily limit ourselves on this point to some of the more simple phenomena.

If, for example, instead of spherical electrodes in spherical glass tubes, cylindrical (wire) electrodes in cylindrical tubes

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[*] Cf. *Galvanismus* (2), Bd. ii. S. 954, u. Fig.

are used, the electrical tension is greatest at their front free ends; in this case also the repulsion of the electricity accumulated round the electrodes on the glass, which spreads with decreasing density, at the ends of the electrodes is relatively least. The discharge, therefore, soon passes under rather high pressure out of these front ends. If the air is more rarefied, the tension necessary for the commencement of the discharge, as well as the charge of the surrounding glass, and also the difference of the tension necessary for the discharge at different parts of the electrodes, are smaller, particularly at the negative electrode, which always requires a smaller charge. The electricity conveyed from the electric machine, or from the inductorium, is sufficient then to produce the discharge onto the negative electrode, and even at a certain distance from its end. On the positive electrode the discharge begins only when the charge is stronger, therefore the differences of charge at the various parts of the electrode are greater, and the discharge passes almost exclusively from the front end of the positive electrode.

If the part of the tube surrounding the negative electrode be narrower, a more powerful charge is necessary for the discharge on the sides of the electrode than if it be wider, on account of the repulsion of the electricity upon the tube. The discharge contracts, therefore, more and more at the end of the electrode, and, in consequence of the repulsion through the negatively charged glass, it can spread a long way towards the positive electrode.

If, further, two tubes, with a small and a large negative electrode respectively, are inserted consecutively in the circuit, a discharge begins as soon as the tension has reached a certain degree on the negative electrode. To attain this a more powerful charge is required at the larger than at the smaller electrode. If, then, the discharge begins at the former, the body of electricity flowing to the latter will be relatively greater than is necessary, the air repelled by the initial discharge will be still more accelerated, and the halo will spread further than from the larger negative electrode.

If two wire electrodes, covered on their lateral surfaces by a glass coating, be fixed axially in a cylindrical tube, one (I.) at the end, the other (II.), bent, from the side towards the centre of the tube, so that it turns its free surface away from the first electrode, and if the electrodes be so far removed from one another that their influence on one another is only slight, the glass around the electrode II., when the latter is negative (particularly if the pressure be low), will be only slightly and almost uniformly charged (according to the feeble

tension necessary for the discharge), the reaction of this charge on the charge of the electrode itself will be relatively small, and the halo round the electrode will occupy a larger space. In consequence of this, the discharge can spread itself in the direction in which the potential diminishes most rapidly, *i. e.* in the direction of the electrode I. If, however, the electrode II. is positive, the charge on the glass opposite to the free surface will be relatively strong, and the negative discharge will extend from the electrode I. to the neighbourhood of the electrode II.; so that the discharge proceeding from the electrode II., in consequence of the repulsion by the first and the attraction by the second, will be bent towards the electrode I. At the same time the halo proceeding from the electrode I. will in the latter case spread itself beyond the positive electrode, because the positive discharge, confined in a limited space and emitted with great velocity from a small spot in the direction of the resultant force, cannot at once neutralize the negative electricity with which every point of the halo is charged.

A similar explanation applies to the beautiful experiment of Hittorf, where two straight wire terminals are inserted at right angles to one another in a sphere of glass. Since when the rarefaction is sufficient, and the influence exerted by the positive electrode not too strong, the negative discharge spreads from a great part of the surface of the negative electrode, and, according to our theory, a little earlier and before the positive discharge at the point of the positive electrode begins, therefore the latter, in order to neutralize its electricity with that of the negative electrode, cannot turn directly towards the negative electrode, but also must be diverted round the latter in the direction of the negatively electrified glowing gas. The same is the case with electrodes parallel to one another; for then the discharge of the positive electrode passes to the spot at which the attraction of the positive and negative electricity in the tube is at its maximum.

If an induction-coil be used, the phenomena are more complicated than with an electric machine. The quantities of electricity driven by induction to the ends of the coil accumulate not only there, but also more or less in the interior. If, therefore, those ends are connected with the electrodes of a tube, and if their electricity has attained to the tension necessary for a discharge, the gas stratum at the electrodes will be thrown off and rarefied by the initial discharge, since the movement of the gas from the sides is slow in comparison with that of the electricity. Thus the electricity flowing to the electrode from the interior of the coil may be partly transmitted

from the point where the discharge has begun, even when the charge on the electrodes does not attain its former density.   In this manner the discharge of the induction-coil may last a shorter or longer time.   The quantity of electricity passing through the tube is greater or less in proportion to the rapidity with which the electric density decreases at the electrodes, while at the same time the discharge is gradually checked by the flow of the denser air at the sides.   When this electric density has fallen to a certain amount the discharge ceases, and the remainder of the electricities neutralize themselves backwards through the coil. The construction of the coil itself, the insertion of resistance between its terminals and the tube, which tend to prolong the period during which the accumulation of electricity lasts, an increase of the surface of the conductors by condensing-plates or other means, may each materially influence the quantity of electricity passing through the tubes.

If a galvanometer be inserted in the circuit, or in a branch of it, the deviation measures directly or indirectly the quantity of electricity which has passed through the tube—*i. e.* the quantity which flowed to its electrodes, less that which flowed back into the coil.   If the electrodes be connected direct, and the deviation of the galvanometer observed, or if the tube be replaced by a rheostat so as to make the deviation the same as before, the backward flow (alternate discharges apart) does not take place.   In the first experiment the antecedent *electrostatic* relations exercise an influence on the quantity of electricity carried over in the discharge, while in the second the flow of the electricity actually set in motion is the only quantity concerned.   The two experiments, therefore, take place under quite different conditions ; so that the resistance of the tube cannot be determined by a comparison of the results [*].   Nor, indeed, can it be effected by connecting a galvanometer with different points of the conductors, so as always to produce the same deviation under various conditions of current and tube [†].

We therefore have to examine in what relations the results already obtained, as to the supposed resistance of the separate parts of the discharge, stand with regard to the conditions of the discharge in general ; and for this purpose we return more particularly to the beautiful experiments of Hittorf [‡], from which we will select the simplest.

[*] Morren ; cf. *Galvanismus* (2), Bd. ii. S. 951.
[†] A. de la Rive; cf. *Galvanismus* (2), Bd. ii. S. 952, 953.
[‡] Cf. *Galvanismus* (2), Bd. ii. S. 958.

a. *Resistance of the positive discharge.*—If two tubes of different lengths are blown on to a glass globe containing the negative electrode, and if at the ends of the tubes two similar wires are placed as positive electrodes, and if the current of induction is passed first through the long tube and then through the shorter, the intensity of the current appears greater in the latter case, because, in consequence of the greater mutual influence of the electrodes on one another, a smaller body of electricity is necessary to produce the tension requisite for a discharge ; the discharge begins earlier, and lasts longer. If, however, in the latter case the flow of electricity is retarded by the insertion of, *e. g.*, a capillary tube with a solution of sulphate of zinc, the discharge again ceases sooner, and the same intensity of current may be obtained as in the longer tube. The resistance of the solution in the capillary tube does not, however, as argued above, directly correspond with the difference of the resistance in the two glass arms.

If in this experiment the rarefaction of the gas be carried still further, the induction-coil will continue to discharge itself, even when the tension has fallen below that which was necessary with a higher pressure ; the quantities of electricity passing between the nearer electrodes become more equal to those passing between the more distant ; and the resistance by which the flow of electricity to the nearer electrodes may be retarded, must be less in order to reestablish a complete equality. The phenomenon is therefore outwardly the same as if the resistance of the positive discharge decreased. In the same way, if the electromotive force of the induced current be more powerful, *e. g.* if a stronger inducing current is employed, or if the interruption of it be more rapid, the quantity of electricity initially flowing to the electrodes will be greater, the electricities flowing back to the coil after the discharge will be less, in proportion to the whole ; and the resistance of the positive discharge will appear, both by measurement and by the above-mentioned experiments, to be less.

b. *Resistance at both electrodes.*—According to Hittorf, if the current is divided between two tubes of equal diameter, but having the electrodes at different distances from one another, the intensity in the two branches approaches equality as the rarefaction is carried further. As in this case the length of the positive discharge varies, Hittorf concludes that the resistance of the former is small compared with that around the negative electrode. It was mentioned at an earlier part of this paper that the greater the rarefaction the smaller the charge on the electrodes necessary for the com-

mencement of the discharge; the influence of the electricities
of the electrodes on one another will therefore, under similar
circumstances, decrease; and the discharge will consequently
depend more exclusively upon the surroundings of the elec-
trodes. Nearly equal quantities of electricity are discharged
in tubes of different lengths.

e. *Resistance at the negative electrode.*—If we were inclined
to infer that the supposed resistance of the positive discharge
compared with the negative is, under certain circumstances,
small, we might determine the supposed resistance of the
halo by measuring the resistance of the tube with different
negative electrodes.

1. *Influence of the size of the electrodes.*—If two similar
tubes with positive electrodes of the same length, and nega-
tive of the same thickness but different lengths, are inserted
side by side in the circuit of the coil, then, according to
Hittorf, when the rarefaction is such as to produce a halo
covering completely both the negatives, the intensity in the
two branches, setting aside the influence of the positive elec-
trode &c., will be nearly in proportion to the length of the ne-
gative electrode. In this case, if the induction-current gives a
quantity of electricity sufficient to maintain the whole surfaces
of the two electrodes at a tension necessary for a discharge, and
therefore under similar conditions when the discharge takes
place, then the quantity of electricity discharged from all their
parts must (last paragraph) be in proportion to their surface.
The final result is therefore the same as it would be on the
supposition that the resistance of a tube was approximately in-
versely proportional to the surface of its negative electrode.
In this, however, we have set aside the influence mentioned
by Hittorf, of the positive electrode and of the greater cooling
of the gas on a very small negative electrode; in this case,
according to our explanations, the gas driven away will be
more quickly replaced, and therefore the passage of electricity
ceases sooner after the beginning of the discharge, and the
resistance appears proportionally too great.

That the same thing does not occur when the tubes are in-
serted separately in the circuit and the intensity of the current
is measured, must arise from the fact that with an equal flow
of electricity the shorter electrode sooner obtains the tension
necessary for the commencement of a discharge than the
longer, consequently the discharge also begins sooner; there-
fore the quantities of electricity which flow through one or
other of the tubes are not so very different from one another.
If the intensity of the current is brought, by the insertion of
tubes of sulphate of zinc instead of vacuum-tubes, to the same

degree, the conditions are fundamentally altered, and a direct comparison of the resistance may give results different from those obtained by the above-mentioned experiments.

2. *Influence of the surrounding spaces.*—If the spaces surrounding the negative electrodes are not alike, different tensions are necessary in order to produce discharge. With cylindrical electrodes of different thickness, but the same length, enclosed in tubes of equal diameter, the thicker electrodes require a greater charge, because the repelling reaction of the homonymous charge of the glass envelope on the electricity of the surface of the electrode is greater; and also the electrode itself, in consequence of its larger surface, requires a greater quantity of electricity to charge it with equal tension at all points. If, when simply connected with a Holtz's machine, we suppose the potentials to be the same at every point of the electrode, the electrode whose thickness is represented by n would receive and discharge at every discharge more than n times the quantity of electricity received and discharged by that whose thickness is represented by unity. If, however, two tubes containing dissimilar negative electrodes are inserted side by side in the circuit of a coil, the relations are much more complicated and not so easily perceived as in the above-mentioned case. The current would divide itself in proportion to the extent of the surfaces; in consequence of the greater density of the electricity on the thicker electrode necessary for the discharge, the discharge at the n times thicker electrode would begin later and cease earlier, and therefore less than n times the quantity of electricity would be discharged through it—provided always that all the electricity did not flow out through the thinner electrode, and also that the electricity is conveyed to the electrodes from the coil with such velocity that a backward current to the thinner electrode of the electricity flowing to the thicker electrode does not take place during the discharge. On the contrary, the gas may be more forcibly driven from the shorter electrode in consequence of the greater charge, whereby a greater body of electricity will follow the initial discharge if there be sufficient gas to convey it.

3. *Influence of pressure.*—If two similar tubes, but containing gas at different pressures, be inserted side by side in the circuit, there is considerable complication in the results.

According to our experiments the tube I. with the denser gas would require a greater charge for the production of the discharge, and consequently the discharge in it would begin later and cease earlier; therefore a less body of electricity would pass through tube I. than through tube II. with the rarefied gas, supposing that the electricity from the coil flowed

equally to both tubes. As, however, in the tube I. the discharge begins with greater initial velocity, more electricity might, in consequence of the rarefaction of the air, follow the first discharge than in the tube II. How far these different conditions balance one another cannot be decided without further investigation. If, further, the tension of the electricity is too feeble to break through the denser gas, all the electricity from the electrodes of the tube I. may eventually flow back to those of the tube II., and eventually to the coil; and in that case no discharge would take place in the denser gas. If, on the other hand, the gas in the tube II. is so much rarefied that it is insufficient to convey the electricity, the discharge can only pass through the tube I. with the denser gas.

If a tube be connected with a constant battery, different relations may occur. If the electromotive force at the poles of the battery be too small to communicate to the electrodes the tension necessary for the commencement of a discharge, no current passes. If the electromotive force is sufficient, or more than sufficient, there ensue discharges which may continue for shorter or longer periods, according to the quantity of the electricity accumulated at the poles and to the velocity of the conduction of the electricity from the battery. Their further continuance depends upon the conductivity of the substances composing the battery. If this is small, the tension on the electrodes diminishes so much at the discharge that the battery cannot offer sufficient tension for the continuance of the discharge; consequently it requires a longer time after each discharge to reproduce the tension at the poles; there ensue single intermittent discharges. If the conductivity of the battery is comparatively great, and if the tension is renewed so quickly that, in the time between the first and the next discharge, the gas rarefied by the discharge at the electrodes has not yet returned to its former condition, the first discharge leads on to the second, and so on in a continuous current, in which the substance of the electrodes eventually participates. But this current differs again from the ordinary galvanic current in that it is essentially dependent upon the impulse which the portion of the gas lying nearest to the electrodes receives from the electricity conveyed to it. The discharge of the latter ensues as when the machine is used as in the experiments mentioned at the beginning of this paper. Thus the "resistance to the discharge," or more properly the quantity of electricity simultaneously discharged, is,

irrespectively of surrounding circumstances, essentially inde-
pendent of the distance between the electrodes.   This result
has been substantially established by Herwig for the passage
of electricity through vapour of quicksilver between quick-
silver electrodes.   The galvanic arc between electrodes of car-
bon or of metal ought also to offer an example of this kind of
discharge.   In this case also no discontinuity can be observed
in the revolving mirror, and the conveyance of the electricity
occurs simultaneously with the conveyance of the particles of
the electrode charged with it, which then, in fact, become va-
porized, as may be seen by the spectrum of the arc.   In this,
as in the other gas-discharges, therefore, the supposed resist-
ance of the arc is only partially dependent upon its length ;
it diminishes in proportion to the quantity of electricity passed
through it in a unit of time, because the electrodes are thus
more heated, and consequently a greater vaporization and
conveyance of electricity are caused.

The formation of the arc must also be preceded by a com-
paratively high tension of the electricity at the electrodes,
whether by their being brought nearer to one another or by
the passage of an electric discharge between them, whereby
the conveyance of matter from the electrode may be effected,
when a lower battery-tension suffices to maintain a continuous
current of electricity.

Resistance in the ordinary " galvanic " sense, however,
would not be offered by a discharge of this kind.   The quan-
tity of electricity conveyed by it would rather depend upon
very various circumstances—upon the medium between the
electrodes, and therefore also upon the facility with which parts
of the electrodes can be thrown off in solid particles or in the
form of gas, upon the nature of the electrodes, the strength of
the current, also upon the quantity and temperature of the par-
ticles conveyed from the electrodes to the space between them,
&c.   These conditions are in many respects analogous to,
though more complicated than, those of the single discharges
from the electric machine.

It may easily happen that between the electrodes, through
the connexion with the poles, a constant column of gas-dis-
charges should take place, at first discontinuously, possibly
with less participation in the matter of the electrodes.   But
when by these means the stratum lying between the electrodes
is heated, rarefied, and eventually filled with the particles of
the electrodes, if the battery conveys the electricity in suffi-
cient quantity, a continuous discharge may take the place of the
discontinuous discharge in the thus altered intermediate space,
in which case, at the same time with the change in the manner

of the discharge, a change occurs in the heating of the electrodes. Such a phenomenon has been observed by Gassiot [*] at the discharge of a Grove battery of 400 pairs between metal or coke balls.

XXV. *On a suggested Explanation of the Obliquity of Planets to their Orbits.* By GEORGE H. DARWIN, *M.A., Fellow of Trinity College, Cambridge*[†].

IN a former paper [‡] I have shown that if θ be the obliquity to its ecliptic of a planet which is slowly changing its shape, so that its principal moments of inertia at the time t are $A + at$, $A + bt$, $C + ct$, then, so long as at, bt, ct remain small compared with $C - A$,

$$\frac{d\theta}{dt} = \frac{\Pi}{2n} \frac{a + b - 2c}{C - A},$$

$\Pi \operatorname{cosec} \theta$ being the precession of the equinoxes, and $-n$ the rotation of the planet. This equation will hold true for long periods, if all the quantities on the right hand are treated as functions of the time; and if $a = b$ it may be written

$$\frac{d\theta}{dt} = -\frac{\Pi}{n} \frac{\dfrac{d}{dt}(C - A)}{C - A}.$$

In the case of the earth,

$$\frac{6\pi^2}{n} \left\{ \frac{1}{T^2} + \frac{1}{T'^2} \frac{1 - \frac{3}{2}\sin^2 i}{1 + \nu} \right\} \frac{C - A}{C} = \frac{\Pi}{\sin\theta\cos\theta} = \frac{p}{n} \frac{C - A}{C}, \text{ suppose,}$$

where T, T' are the year and month, ν is the ratio of the earth's mass to the moon's, and i is the inclination of the lunar orbit to the ecliptic. In the corresponding function for any other planet there will be a term for each satellite, and $1 - \frac{3}{2}\sin^2 i$ will be replaced by a certain function called λ by Laplace.

The equation may now be written

$$\frac{Cn}{p} \frac{d\theta}{dt} \log\tan\theta = -\frac{1}{n} \frac{d}{dt}(C - A).$$

The object of the present note is to apply this equation to the supposition that the planets were originally nebulous masses, and contracted symmetrically under the influence of the

[*] *Galvanismus* (2), Bd. ii. S. 1044.
[†] Communicated by the Author.
[‡] "On the Influence of Geological Changes on the Earth's Axis of Rotation," Abstract, Proc. Roy. Soc. No. 175 (1876).

mutual gravitation of their parts. This application involves a large assumption, viz. that the precession of a nebulous mass is nearly the same as though it were rigid. In defence thereof I can only quote Sir W. Thomson, who says, " Now, although the full problem of precession and nutation, and what is now necessarily included in it—tides, in a continuous revolving liquid spheroid, whether homogeneous or heterogeneous, has not yet been coherently worked out, I think I see far enough towards a complete solution to say that precession and nutations will be practically the same in it as in a solid globe, and that the tides will be practically the same as those of the equilibrium theory " *.

I therefore once for all make this assumption.

The coefficient p depends solely on the orbit of the planet and of its satellites, and during the contraction of the mass will have been constant, or very nearly so. To determine the other quantities involved, we have the three following principles:—

(1) The conservation of angular momentum.
(2) The constancy of mass of the planet.
(3) That the form of the planet is one of equilibrium.

(1) is expressed by the equation $Cn = H$, a constant ; and, if ρ, a be the mean radius and density of the planet at any time, (2) by $\frac{4}{3}\pi\rho a^3 = M$, the mass. Then, if the law of internal density during contraction be that of Laplace, viz. $\frac{Q \sin qr}{r}$, if k be the ratio of the surface-density to the mean density, e the ellipticity of the surface, and m the ratio of the centrifugal force at the distance a to mean pure gravity, the third principle gives †

$$\frac{5m}{2e} = \frac{(qa)^2}{3k(qa-1)} - 3k.$$

Also

$$C = \frac{2}{3}\left\{ 1 + \frac{6}{(qa)^2}(k-1) \right\} Ma^2,$$

$$C - A = \frac{2}{3}\left(e - \frac{m}{2} \right) Ma^2,$$

$$m = \frac{3n^2}{4\pi\mu\rho}.$$

* Address to Section A. of the British Association at Glasgow, 'Nature,' September 14, 1876, p. 429.

† Compare Thomson and Tait's 'Natural Philosophy,' § 824 (14), § 827 (20).

Hence (1), (2), and (3) lead to the following equations:—

$$\frac{2}{3}\left\{ 1+\frac{6}{(qa)^2}(k-1)\right\} \mathrm{M}a^2 n=\mathrm{H}, \quad . \quad . \quad . \quad (4)$$

$$\rho a^3=\frac{3\mathrm{M}}{4\pi}, \quad . \quad . \quad . \quad (5)$$

$$\frac{n^2}{4\pi\mu\rho}\left\{ \frac{5}{\dfrac{(qa)^2}{3k(qa-1)}-3k}-1\right\} \mathrm{M}a^2=\mathrm{C}-\mathrm{A}. \quad . \quad (6)$$

If during contraction qa remains constant, and if the coefficient of $\mathrm{M}a^2 n$ in (4) be called γ, and that of $\dfrac{\mathrm{M}a^2 n^2}{4\pi\mu\rho}$ in (6) be called β, then it will be found that

$$\frac{1}{n}\frac{d}{dt}(\mathrm{C}-\mathrm{A})=-\frac{\mathrm{H}\beta}{\gamma}\frac{1}{12\pi\mu\rho^2}\frac{d\rho}{dt}.$$

Hence, remembering that $\mathrm{C}n=\mathrm{H}$,

$$\frac{d}{d\rho}\log\tan\theta=\frac{p\beta}{12\pi\mu\gamma\rho^2}.$$

Integrate, and let D, I be the present values of ρ and θ; then

$$\log\frac{\tan\theta}{\tan\mathrm{I}}=\frac{p\beta}{12\pi\mu\mathrm{D}\gamma}\left(1-\frac{\mathrm{D}}{\rho}\right).$$

If we assume that qa has always the same value as it now has in the case of the earth [*],

$$\gamma=\cdot 3344, \quad \beta=\cdot 9507, \quad \text{and } \frac{\beta}{\gamma}=2\cdot 8433.$$

If during contraction the planet were always homogeneous, the factor $\dfrac{\beta}{\gamma}$ would be replaced by $\dfrac{15}{4}$, or $3\cdot 75$.

Let K stand for $2\cdot 8433$, or $3\cdot 75$, as the case may be; let $Q=\dfrac{1}{\mathrm{T}^2}+\Sigma\dfrac{\lambda}{\mathrm{T}'^2(1+\nu)}$; let P be the periodic time of a pen-

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[*] In determining the precessional constants of Jupiter and Saturn, Laplace assumed that their law of internal density was the same as that of the earth. The assumption is, I believe, unjustifiable; but it will give sufficiently good results for the present purpose. The limiting value of $\dfrac{\beta}{\gamma}$, when the surface-density is infinitely small, and if the Laplacian law still holds good, is $1\cdot 99$. See 'Monthly Notices of the Royal Astronomical Society,' December 1876.

dulum of length equal to the present mean radius of the planet, swinging under mean pure gravity. Then

$$\frac{p}{2\pi\mu\mathrm{D}} = \frac{\mathrm{P}^2\mathrm{Q}}{6},$$

and the equation becomes

$$\log\frac{\tan\theta}{\tan\mathrm{I}} = \frac{\mathrm{KP}^2\mathrm{Q}}{6}\left(1 - \frac{\mathrm{D}}{\rho}\right).$$

This equation shows that as ρ diminishes θ diminishes, and when ρ is infinitely small θ is zero. That is to say, if a nebulous mass is rotating about an axis nearly perpendicular to the plane of its orbit, its equator tends to become oblique to its orbit as it contracts.

In the case of the earth, $\mathrm{P}^2\mathrm{Q} = \dfrac{8\cdot5577}{10^8}$; and taking the present obliquity of the ecliptic as $23°\ 28'$, the equation may be written

$$\mathrm{Log}_{10}\tan\theta = 9\cdot63761 - \frac{1\cdot7612}{10^8}\cdot\frac{\mathrm{D}}{\rho}$$

On the hypothesis of homogeneity, $1\cdot7612$ must be replaced by $2\cdot3229$.

The extreme smallness of the coefficient of $\dfrac{\mathrm{D}}{\rho}$ shows that the earth must have had nearly the same obliquity even when its matter was rare enough to extend to the moon. But if it can be supposed that the moon parted from the earth without any abrupt change in the obliquity of the planet to the ecliptic, then from that epoch backwards the function Q would have had only one term, viz. $\dfrac{1}{\mathrm{T}^2}$, and $\mathrm{P}^2\mathrm{Q}$ would be $\dfrac{2\cdot5750}{10^8}$. The coefficient of $\dfrac{\mathrm{D}}{\rho}$ in the above equation would be reduced to $\dfrac{5\cdot30}{10^9}$, or $\dfrac{7\cdot00}{10^9}$, according to whichever value of K is taken. This being granted, it follows that when the diameter of the earth was 1000 times as large as at present, the obliquity to the ecliptic was only a few minutes.

This somewhat wild speculation can hardly be said to receive much support from the cases of the other planets; but it is not thereby decisively condemned. In all the planets up to and inclusive of Jupiter, the expression Q will have to be reduced to its first term $\dfrac{1}{\mathrm{T}^2}$, because the satellites are rather near their

primaries.   Hence one would expect that the obliquities of the
planets to their orbits would diminish as we go away from the
sun.   It is believed (but the observations seem doubtful) that
Mercury and Venus are very oblique to their orbits ; and Mars
has an obliquity nearly the same as that of the earth.   The
region of the asteroids is a blank ; and then we come to Ju-
piter, with a very small obliquity.

The next in order is Saturn : and his case is unfavourable ;
for he is slightly more oblique to his orbit than is the earth.
Nevertheless it must be observed that he has a large number
of satellites, and some are very remote from him, and his mean
density is very small ; hence, if the satellites can have affected
the obliquity in any case, one would expect them to have done
so in that of Saturn.

No light whatever is thrown on the case of Uranus, whose
axis is said to lie nearly in the plane of his orbit.

XXVI. *On the Magnetization of Steel by Currents.*
By E. Bouty, *Docteur ès Sciences.*

[Concluded from p. 135.]

III. *Temporary and Permanent Magnetization of thin Needles
only slightly hardened.*

THESE experiments, like the foregoing, were made with
the steel wire used for spindles in clock-making ; but
instead of steeping the needles at a red-heat, they were used
in the state in which they were delivered by the maker—that
is to say, not tempered.   Here rupture-experiments are out of
the question, and it is convenient to take as a starting-point,
not the study of the permanent magnetization, but that of the
temporary.

The needles to be investigated are placed at a distance from
the galvanometer-needle equal to 50 or 60 centims. ; and the
operation is conducted according to the method employed in
the preceding section[*].   At this distance, and with the dimen-
sions of the needles used, Gauss's formula applies without
needing the introduction of the corrective terms, and one can
compare the moments of needles of different lengths without
knowing any thing *à priori* of the position of the poles.   Let
us consider n needles of different lengths $l, l', \ldots$, and submit

* The quantity of magnetism being much greater in feebly hardened
needles than in the same needles strongly hardened, the employment of
long distances does not diminish the absolute values of the deflections
sufficiently to render the measurements uncertain in the present case, as it
did in that of the preceding section.

them to the action of the same magnetizing force; let $\dfrac{X}{2}$ be the distance from one pole to the near extremity in any needle; m, m', m'', ... the observed moments; we shall have

$$\frac{m}{l-X} = \frac{m'}{l'-X} = \frac{m''}{l''-X} = \cdots \quad \cdots \quad \cdots \quad (1)$$

These equations permit us to verify that X is constant, and to obtain its approximate value. It is found that X does not vary, whatever may be the intensity of the magnetizing force.

For needles of 0·920 millim. diameter $\dfrac{X}{2} = 10\cdot33$ millims. has been found; and in the same needles tempered hard, $\dfrac{X}{2} = 4\cdot6$ millims. Therefore the quantity X is connected with the state of the steel; it depends on its magnetic conductivity, as will be seen in the course of this memoir.

The *Temporary-Magnetism Function.*—The polar distance being known, the curve which represents this function can be constructed, just as in the case of hardened needles. The following Table was obtained for needles of 0·526 millim. diameter. The second column refers to the temporary magnetism, the third to the permanent; the last gives the value of the ratio r of these two quantities.

Current.	T.	P.	r.
3·87	1·003	0·300	3·343
5·44	1·589	0·871	1·867
6·57	2·416		
8·00	3·375	2·297	1·891
9·87	3·702		
11·56	4·434		
14·84	5·017	3·073	1·633
20·25	5·487	3·265	1·681
32·50	6·704	3·731	1·796
43·75	6·449		
60·37	7·543	3·819	1·975

The preceding Table gives occasion for numerous remarks. In the first place, we observe that the magnetization, whether temporary or permanent, begins to be sensible at very low values of the magnetizing force, which did not take place in the case of the very hard-tempered steel; the point of inflection of each curve is also nearer the origin; and beyond it the curves, especially those which relate to the temporary magnetism, approach their asymptote much more slowly. In a word, the variation of the quantities of magnetism takes

place with much less steepness, and the curves referring to it present less analogy with simple broken lines, such as would be given by the old theory of the coercive force. This character also presents itself most distinctly in the curves given by Rowland for various sorts of iron, contrary to the results furnished by Bessemer steel.

Let us also notice that the ratio r, which we have found decreasing towards a certain limit when the intensity of the magnetizing force increased indefinitely, presents in the case of soft-tempered steel a very definite minimum, beyond which r augments very sensibly. Although this minimum presents itself here for the first time in my experiments, its existence appears to be pretty general; I find it in all the experiments made by Rowland upon iron, steel, nickel, and cobalt—although with special characters, according to the state and the nature of the substance * :—

		F.	r.
Norway iron, magnetic ...	$\Big\{$	0·2673	3·875
		0·9305	1·350 minimum.
		36·86	2·099
Burden's "best," normal...	$\Big\{$	0·1456	3·823
		1·655	1·366 minimum.
		36·60	2·073
Burden's "best," burnt ...	$\Big\{$	0·553	2·193
		1·317	1·243 minimum.
		51·03	1·511
Bessemer steel, normal ...	$\Big\{$	0·1356	18·176
		7·495	1·358 minimum.
		38·99	1·523
Stubb's steel wire	$\Big\{$	1·084	12·47
		20·54	1·519 minimum.
		38·64	1·692
Cast nickel, normal.........	$\Big\{$	2·136	2·394
		5·900	1·472 minimum.
		82·92	2·227
Cast cobalt, normal.........	$\Big\{$	3·925	7·160
		21·06	2·452 minimum.
		117·00	3·244

The limits within which are comprised the magnetizing forces employed by me are narrower than those attained by the American savant; and it appears to me very probable that the minimum of r would have been clearly marked, even in the case of very hard-tempered steel, if the upper limit of F

* The values of r have been calculated from the numbers given by Rowland in the two papers published in the 'Philosophical Magazine,' August 1873, and November 1874.

had been sufficiently high in my experiments. The value of F corresponding to the minimum of r is much lower in the case of soft tempering; hence the facility with which its existence is recognized in the latter case.

IV. *Experiments on Cylindrical Bars.*

The preceding experiments were directed so as to permit the investigation of the magnetizing function and the direct determination of the situation of the poles; the object of the present has been to supply the characteristic constants of the distribution of the magnetism in very various circumstances. It will be seen that both problems depend, in the last analysis, on the determination of the same physical elements.

1. I have already established[*] that the magnetic moments y of thin cylindrical needles, tempered hard and magnetized to saturation, are represented by the formula

$$y = \mathrm{A}a^2 \left(x - \frac{2}{\beta} \frac{e^{\beta\frac{x}{2}} - e^{-\beta\frac{x}{2}}}{e^{\beta\frac{x}{2}} + e^{-\beta\frac{x}{2}}} \right) \text{ with } \beta = \frac{\mathrm{B}}{a}, \quad . \quad . \quad (1)$$

first given by Biot, and connected by Green with the theory of coercive force. I have also shown[†] that the same formula applies to needles derived by breaking from the centre of needles not saturated. At the same period some incomplete experiments[‡] had caused me to suppose that the formula was not applicable to bigger needles; and this induced me to resume, in the case of the cylindrical bars of 6–10 millims. diameter[§], a series of analogous researches, the results of which I now publish.

The measurement of the magnetic moments was effected by the method applied in the second and third sections of the present memoir. The necessity of sometimes operating on bars of considerable length has demanded some special arrangements, which I will briefly indicate. The coil employed to produce the magnetization consists of a zinc tube 1·2 metre long and 5 centims. in diameter, carrying a kilometre of insulated copper wire wound in seven superposed layers. The winding is arranged so that either the whole or only a portion of the coil can, at will, be employed. A horizontal railway, in wood, movable about a vertical axis which passes through the prolongation of the wire of the galvanometer-needle ‖, is

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* *Annales de l'École Normale Supérieure,* t. iii. p. 34.
† Ibid. p. 43. ‡ Ibid. p. 44.
§ French steel, wire-drawn, in rods of two metres length, from MM. Peugeot and Jackson. These steels are extremely homogeneous.
‖ The needle is rendered nearly astatic with the help of a compensating bar: see p. 132.

placed perpendicular to the magnetic meridian. The coil is mounted on four rollers, and can be removed at pleasure ; it carries an index, which slides along the rail upon a divided rule, the zero of which coincides with the axis of rotation. By an appropriate arrangement the bars are kept exactly in the axis of the coil.

It has been verified that the deflection produced by a bar of 50 centims. length is exactly proportional to the cube of the distance of its centre from the needle as soon as that distance exceeds 2 metres: in general a much greater distance was taken.

Under these conditions I found that, for suitably selected values of A and B, formula (1) will always represent the magnetic moments exactly, provided that all the bars, innocent of previous magnetization, be magnetized in identical circumstances.   It applies to the temporary as well as the permanent magnetization, to bars of soft iron as well as to bars strongly hardened, and whether one magnetizes to saturation or excites the coil with a current of constant intensity too feeble to saturate.

The following Tables are intended to show, in some examples, the degree of accordance between experiment and the formula*:—

Bars saturated, 7 millims. in diameter, not hardened.

Length.	Temporary magnetization.			Permanent magnetization.		
	Observa-tion.	Calcula-tion.	Differ-ence.	Observa-tion.	Calcula-tion.	Differ-ence.
centim.						
5·0	0·75	0·758	−0·008	0·056	0·074	−0·018
7·2	2·13	2·062	+0·068	0·187	0·222	−0·035
9·8	4·12	4·368	−0·248	0·469	0·547	−0·078
12·4	7·13	7·852	−0·722	0·845	1·068	−0·223
15·0	12·54	11·896	+0·644	1·726	1·693	+0·033
19·8	20·25	20·358	−0·108	3·440	3·517	−0·077
25·0	30·19	30·260	−0·070	6·328	6·847	−0·519
30·0	37·72	40·087	−2·367	10·91	10·524	+0·386
34·5	48·96	49·037	−0·067	15·18	14·812	+0·368
40·0	......	......	......	21·81	21·81	0·000
	$A_1 = 4 \cdot 081, \quad B_1 = \dfrac{1}{7 \cdot 142}.$			$A_2 = 2 \cdot 34, \quad B_2 = \dfrac{1}{17 \cdot 857}.$		

* I think it expedient to apprise the reader that the numbers representing, in arbitrary units, the measured magnetic moments in this section are always evaluated with the same unit, which is several hundred times as great as that made use of in the preceding sections.   As the corresponding experiments were made, the one series at Rheims, the other at Paris, the data for establishing the *exact* value of the ratio of the two units are wanting.

Bars not saturated, 10 millims. in diameter, not hardened.

Length.	Temporary magnetization.			Permanent magnetization.		
	Observation.	Calculation.	Difference.	Observation.	Calculation.	Difference.
centim.						
11·6	2·60	2·121	+0·479			
15·0	3·63	3·828	−0·198			
19·2	6·68	6·712	−0·032	1·223	1·198	+0·025
24·5	11·47	10·540	+0·930	2·573	2·357	+0·216
29·4	16·17	15·520	+0·650	4·149	3·781	+0·368
34·5	21·75	20·362	+1·388	5·233	5·659	−0·426
39·4	23·44	25·090	−1·650	7·571	7·788	−0·217
	$A_1 = 0.9966,\quad B_1 = \dfrac{1}{7\cdot142}.$			$A_2 = 0.723,\quad B_2 = \dfrac{1}{17\cdot857}.$		

The results furnished by steel not hardened, to which the preceding Tables relate, are particularly interesting.

In the first place, the coefficient B is independent of the intensity of the magnetizing current. A determined value

$$B_1 = \frac{1}{7\cdot142}$$

corresponds to the temporary magnetism; to the permanent magnetization corresponds a much smaller value,

$$B_2 = \frac{1}{17\cdot857}.$$

This coefficient does not vary with the diameter of the bars. It is easily seen, on making x very great in formula (1), that $\dfrac{1}{\beta} = \dfrac{a}{B}$ represents the distance of one pole from the neighbouring extremity in long bars. For $a = 1$ centim. we have

$$\frac{1}{\beta_1} = 7\cdot142 \text{ centims.}, \quad \frac{1}{\beta_2} = 17\cdot857 \text{ centims.};$$

and these polar distances are characteristic of the steel used—the one for the temporary, the other for the permanent magnetization.

Secondly, the coefficient A_1, which is referred to the temporary saturation, is greater than the coefficient A_2, corresponding to the permanent saturation. In general A_1 and A_2 are functions of the intensity of the magnetizing force not different from those which we have studied in sections I., II., and III., under the name of *functions of temporary* and *permanent magnetism.* But the experiments on bars have not

been conducted with the view of studying the variation of these functions.

Tempered bars furnish analogous results, provided that only bars of the same temper be considered. For them the values of A_1 and A_2 are lower than for natural bars: the value of B_1 for the temper which I used becomes equal to $\frac{1}{5}$, and remains constant whatever the intensity of the magnetizing force may be. But the value of B_2 increases with the intensity of that force; its value commences at $\dfrac{1}{13 \cdot 75}$, and amounts to $\frac{1}{8}$ for the most intense magnetizing forces employed by me. These results are of the same sort as those obtained for the values of $d = \dfrac{2a}{B_2}$ in section I.

Let us now pass to the inductions to be derived from the direct results obtained from the study of the moments, relative to the distribution. If it be admitted that the quantity of free magnetism contained in a slice of the thickness du, situated at a distance u from the centre of any bar of the series studied, may be represented by the formula

$$z = \mathrm{A}a^2\beta \frac{e^{\beta u} - e^{-\beta u}}{e^{\frac{\beta x}{2}} - e^{-\frac{\beta x}{2}}}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

the magnetic moment y will be represented by formula (1). The same letters in the two formulæ refer to the same quantities.

Inversely we admit with Biot [*] that, if the magnetic moments verify formula (1), the distributions are conformable to formula (2). The foregoing experiments supply the values of the constants A and B characteristic of the distribution.

To explain to ourselves exactly the physical character of these constants, let us consider the case of a bar of infinite length. In this case the distribution will be represented, starting from the end of the bar as origin, by the formula

$$z = \mathrm{A}a^2\beta e^{-\beta u} = \mathrm{AB}ae^{-\frac{\mathrm{B}}{a}u}. \quad \cdot \quad \cdot \quad \cdot \quad (2\,\mathrm{A})$$

If A alone be varied, the distribution-curve will remain similar to itself, its *form* will not be modified; this is what happens in the case of the temporary magnetization produced by currents of different intensity. If, on the contrary, we make B

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[*] This inference is not a necessary one *à priori*. An infinity of different magnetic distributions may be imagined, giving the same values of the magnetic moments; but formula (2) having been verified directly by various observers, the mode of reasoning we have borrowed from Biot may be with perfect security employed.

to vary, the greater the value of B the more quickly does the distribution-curve approach the axis of the abscissæ, for one and the same value of the extreme ordinate. When B increases, the magnetic distribution *shortens*; it *lengthens* when B diminishes. Therefore the temporary magnetization is *shorter* than the permanent [*].

With M. Jamin, we may call A the coefficient of *capacity*, $\frac{1}{B}$ the coefficient of *conductivity*, of the steel under investigation. The measure of the distance from the poles to the neighbouring extremity of a long bar furnishes the absolute value of its coefficient of conductivity under the conditions of the magnetization. The *temporary conductivity* is invariable, whatever may be the intensity of the magnetizing force; the *permanent conductivity* is so only in the case of steel not tempered[†].

2. After investigating the distribution in a bar innocent of prior magnetization, I sought to apply the same method to bars already magnetized, submitted a second time to the magnetizing action. I selected the simplest case—that of bars not tempered, primitively saturated.

Action of a direct current.—A second application of a direct magnetizing force does not alter the permanent moment of a saturated bar; but, however feeble this force may be, while submitted to its action the bar acquires a temporary moment superior to its permanent moment.

Moreover it is found to be impossible to represent these temporary moments by a single formula of the form of equation (1); on the other hand, one succeeds very well on taking the two-term formula

$$y = c_1 A_1 \left(x - \frac{2}{\beta_1} \frac{e^{\frac{\beta_1 x}{2}} - e^{-\frac{\beta_1 x}{2}}}{e^{\frac{\beta_1 x}{2}} + e^{-\frac{\beta_1 x}{2}}} \right) + c_2 A_2 \left(x - \frac{2}{\beta_2} \frac{e^{\frac{\beta_2 x}{2}} - e^{-\frac{\beta_2 x}{2}}}{e^{\frac{\beta_2 x}{2}} + e^{-\frac{\beta_2 x}{2}}} \right), (3)$$

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[*] No constant force, therefore, can produce upon a bar a distribution of the same form as the permanent distribution. This result is in opposition to the theory of the coercive force.

[†] The number of functions or constants necessary for the complete magnetical definition of a given steel is, as we see, pretty considerable. The two magnetism-functions A_1 and A_2 (or at least the characteristic constants of the rapid magnetization) must be known, and the two conductivity-functions B_1 and B_2. Again, we thus define the state of the steel *as it is*; and the entire examination has to be gone through afresh for each degree of tempering.

in which A_1 and A_2, β_1 and β_2 have the characteristic values of the temporary and the permanent magnetization to saturation; moreover c_1 and c_2 are determined by the following conditions:—

(1) The first term, containing the *temporary* coefficients, represents the total temporary moment which the bar innocent of magnetization would receive from a first application of the magnetizing force F employed.

(2) The second term is the difference between the permanent moment of saturation and the permanent moment which the force F is capable of producing. Thus, let α_1 and α_2 be the temporary and permanent *capacities* of the bar for the force F, we have

$$\left.\begin{array}{l} c_1 A_1 = \alpha_1, \\ c_2 A_2 = A_2 - \alpha_2. \end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

Applying Biot's method of reasoning, we shall be led to represent the distribution of the magnetism, in the bars considered, by the formula

$$z = \alpha_1 a^2 \beta_1 \frac{e^{\beta_1 u} - e^{-\beta_1 u}}{e^{\frac{\beta_1 x}{2}} + e^{-\frac{\beta_1 x}{2}}} + (A_2 - \alpha^2) a^2 \beta_2 \frac{e^{\beta_2 u} - e^{-\beta_2 u}}{e^{\frac{\beta_2 x}{2}} + e^{-\frac{\beta_2 x}{2}}}. \quad \cdot \quad (5)$$

The magnetic distribution is the superposition of two distributions:—the one *short* (temporary), equal to that which a first application of the force F would produce; the other *long* (permanent), equal to the residue of the primitive distribution diminished in quantity by the portion of permanent magnetism corresponding to F. The curious part of the matter is, that the totality of the short distribution is not borrowed from the primitive permanent distribution: new magnetism is called up from the molecular depths to form the difference $\alpha_1 - \alpha_2$; while a portion (often very considerable) $A_2 - a_2$ of the primitive magnetization remains distributed after the *permanent* fashion (long distribution), as if insensible to the action of the magnetizing force. This fact seems to me highly important in regard to the theory of magnetism.

In the following Tables examples are given of the application of formulæ (3) and (4). They refer to bars of 1 centim. diameter.

Length.	Moment of saturation.		Moment of remagnetization.		Difference.
	Temporary.	Permanent.	Observed.	Calculated.	
centim.					
39·6	99·085	33·95	56·834	56·834	0·000
34·4	82·214	22·45	42·391	43·169	−0·778
29·4	60·729	15·38			
24·8	43·730	8·92	21·478	22·166	−0·688
22·1	29·066	6·91	14·635	15·127	−0·492
19·6	23·422	5·13	12·033	11·883	+0·150
15·6	10·712	1·86	6·148	5·160	+0·988

c_1 observed .... 0·40024

c_2 observed .... 0·52528   calculated. .... 0·52924

The values of c_1 and c_2 were measured by the magnetization of bars of the same sort as those of the experiment, but magnetized for the first time. The "calculated" value of c_2 is that which had to be employed concurrently with c_1 to obtain the numbers of the fourth column.

The following is a summary of an entire experiment for five different values of F:—

c_1 observed.	c_2 observed.	c_2 calculated.
0·03688	0·9625	0·9630
0·05123	0·9370	0·9370
0·13113	0·8418	0·8826
0·77836	0·0770	0·0232

These results present a satisfactory general accordance with those which M. Jamin obtained by measuring the extreme ordinates with the aid of the trial-contact[*].

Action of an inverse current on a saturated cylindrical bar.— In this case the bar preserves, after the cessation of the current, a positive or negative permanent moment which is very well represented by the formula (1), with the value B_2 of B characteristic of the permanent magnetization. It has not been possible to formulate by a simple law the relation of the capacity-coefficient $c_2 A_2$ to the corresponding permanent capacity α_2; the diminution $A_2(1-c_2)$ is much greater than α, and appears to approach the limit $2\alpha_2$ assigned by M. Jamin when successively greater forces are considered.

The moment of the bars during the action of the current can only be represented by a formula with two terms—

$$y = c_2 A_2 \left(x - \frac{2}{\beta_2} \frac{e^{\frac{\beta_2 x}{2}} - e^{-\frac{\beta_2 x}{2}}}{e^{\frac{\beta_2 x}{2}} + e^{-\frac{\beta_2 x}{2}}} \right) - c_1 A_1 \left(x - \frac{2}{\beta_1} \frac{e^{\frac{\beta_1 x}{2}} - e^{-\frac{\beta_1 x}{2}}}{e^{\frac{\beta_1 x}{2}} + e^{-\frac{\beta_1 x}{2}}} \right). \quad (6)$$

[*] See *Comptes Rendus de l'Acad. des Sciences*, t. lxxx. p. 417 *seqq.*

The value of c_2A_2 is equal to the value fixed by the above-mentioned experiments; while c_1A_1 is, as in the case of remagnetization, equal to α_1.

The corresponding distribution-formula,

$$z = c_2A_2a^2\beta_2\frac{e^{\beta_2 u} - e^{-\beta_2 u}}{e^{\frac{\beta_2 x}{2}} + e^{-\frac{\beta_2 x}{2}}} - \alpha_1 a^2\beta_1\frac{e^{\beta_1 u} - e^{-\beta_1 u}}{e^{\frac{\beta_1 x}{2}} + e^{-\frac{\beta_1 x}{2}}},$$

presents a singular peculiarity: it represents the superposition of a *long* distribution and a shorter inverse distribution. The resulting form of the distribution differs profoundly from that of the ordinary distribution.

Fig. 4.   Fig. 5.   Fig. 6.

AP > AT.   AP = AT.   AP < AT.
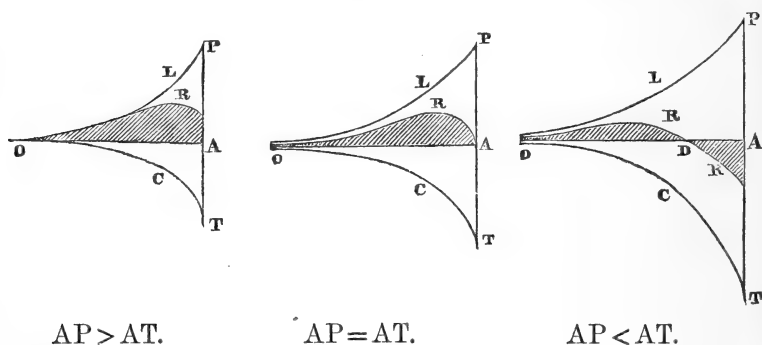
The above curves are intended to exhibit to the eye the principal cases. With suitable values of the force F the distribution-curve cuts the axes of the abscissæ, as is seen in fig. 6, and comprises two regions with opposite signs. The bar that carries this distribution presents two consequent points D, symmetrical in relation to its centre O.

This consequence of the formulæ of the moments is too curious for me not to have sought to illustrate it by direct experiments. For this purpose I regulated the intensity of the demagnetizing current in such a manner that the action of a bar previously saturated was *nil* upon a distant point—that is, that the position of equilibrium of my galvanometer-needle was the same, whether the bar was or was not inserted in the helix. In one experiment the saturated bar, 40 centims. in length and 1 centim. in diameter, produced a positive deviation of 102 divisions of the scale, and after the cessation of the current the needle deviated further in the same direction 8 divisions. The action during the passage of the current was under 0·25 of a division, which is exactly the limit of the errors of read-

ing.   The magnetic moment, then, was sensibly zero.   I then slid the coil along its railway, and, by aid of a small needle very strongly hardened, and rendered sensibly astatic by a compensating bar, ascertained the existence of energetic opposite polarities at the very extremity of the bar and a little nearer its middle.   This delicate but absolutely conclusive experiment, thanks to the regularity and symmetry of the results obtained, cannot leave any doubt of the legitimacy of the induction which guided us in this portion of our investigation.

In brief, these last experiments seem to make it evident that there exists between the temporary and the permanent magnetization a sort of independence more decided than had hitherto been admitted.   I persist in thinking that it must be attributed, at least in part, to a difference of origin, resulting from a real magnetic heterogeneity in the steel[*].

XXVII. *On the Nebular Hypothesis.*—V. *Æthereal Nodes.* *By* Pliny Earle Chase, *LL.D., S.P.A.S., Professor of Philosophy in Haverford College.*

ONE of the most important corollaries of the theory of universal gravitation is tersely stated by Stockwell[†] as follows:—" The amount by which the elements of any planet may ultimately deviate from their mean values can only be determined by the simultaneous integration of the differential equations of those elements, which is equivalent to the summation of all the infinitesimal variations arising from the disturbing forces of all the planets of the system during the lapse of an infinite period of time."   Therefore, within the limits of secular eccentricity, the result is the same *as if* the nebular hypothesis were true.

There should, then, be tendencies, in the neighbourhood of every inert particle which floats in an elastic medium, to the formation of harmonic nodes of various kinds ; and the sum of such tendencies should fix loci of cosmical aggregation before there had been any considerable shapings of definite mass. The subsequent values of relative mass would depend upon mutual conditions of equilibrium between various forms of living force.   But such accordances as would be thus produced, however interesting and however striking they might be deemed, would furnish no more conclusive evidence of the nebular theory, as popularly interpreted, than of the Cartesian

[*] See *Annales de l'École Normale Supérieure,* t. iii. p. 52.
[†] Smithsonian Contributions, 232. viii.

vortices. All assumptions as to the nature of ultimate physical force are now, and perhaps always will be, mere assumptions. Still, like geometrical diagrams, they may help to fix the mind upon ultimate physical resultants, and thus serve a useful purpose.

The velocity of rotation varies as the square root of the velocity of gravitating fall. Therefore, if the velocity of planetary revolution ($\sqrt{gr}$) at Sun's equatorial surface be taken to represent the velocity of æthereal rotation at the same point, the present æthereal atmospheric limit, at which the equatorial velocity of rotation would be equal to that of the æthereal undulations which drive particles towards centres of inertia, is near the outer limit of the asteroidal belt (at $\sqrt{\text{light-modulus}}$ $= 688 \cdot 3$ solar radii $= 3 \cdot 204$ mean vector-radii of Earth). The mean proportional between Earth's mean distance and Saturn's secular aphelion is $3 \cdot 216$. . . . . (1)

We have found * that the velocity at the æthereal atmospheric limit (the velocity of light) is the limiting radial velocity at the point of equilibrium between the velocity of complete dissociation and the velocity of incipient aggregation. The limiting tangential velocity, at the point of equilibrium between complete aggregation and incipient dissociation, is therefore† $\dfrac{1}{\pi}$ of the velocity of light, or the velocity of æthereal rotation at $219 \cdot 09$ solar radii, Earth's mean distance being $214 \cdot 86$. . . . . (2)

We have also found ‡ that Jupiter, the largest extra-asteroidal, and Earth, the largest intra-asteroidal planet, are connected by a common limiting radial velocity, the indications pointing to nucleal or rotating influences in the case of Earth, and to atmospheric or orbital influences in the case of Jupiter. Circular-orbital velocity varies as $g^{\frac{1}{4}}$. The relations of g to $\sqrt{\text{M}}$ (1), and of Earth's position to the unit of solar aggregation (2), lend importance to the approximate equality between $\log (\sqrt{\text{M}})^{\frac{1}{4}} = \cdot 709444$ and log rad. vec. $(♃ \div ⊕) = \cdot 716237$. (3)

Alexander showed § that Jupiter and Saturn are so related as to suggest a possible early mutual participation in a common nucleal *vis viva*, as if they had been formed, by interior and exterior condensation, from the same nebulous belt. I have shown ‖ that the atmospheric radius varies as the $\frac{4}{3}$ power of

* "Correlations of Central Force," Phil. Mag. September 1876, η, ι.
† Ibid. ζ.   ‡ Ibid. λ.
§ Smithsonian Contributions, 280, p. 38.
‖ Correlations, κ, ν. The present value of modulus in solar radii is given in ν.

the nucleal radius. Therefore if $M^{\frac{s}{3}}$ represents Jupiter's position (3) at the extremity of a nucleal radius, the corresponding atmospheric radius $=(\sqrt[s]{M})^{\frac{4}{3}}=M^{\frac{1}{6}}$, of which the logarithm is ·945926. The logarithm of ♄ secular perihelion $\div$ ⊕ mean radius vector is ·941236. . . . . (4)

The secular perihelion of Venus is near the æthereal nucleal limit (1), (4). Log $(\sqrt{M})^{\frac{3}{4}}=2\cdot128332$; log ♀ secular perihelion $= 2\cdot159680$; $2\cdot159680 - 2\cdot128332 = \cdot031348 = $ log $1\cdot0748$; ♀ secular aphelion $\div$ mean radius vector $=$ ·0763. . . . . (5)

These approximations point to æthereal influences on the principal planets, both in the supra- and in the infra-asteroidal belt, and to early special nucleal condensation in the inner belt. The latter indication is strengthened by the greater density of the interior planets, by the many harmonic relations which are based on Earth's distance as a primitive unit, and by Earth's position near the centre of the infra-asteroidal belt. Mercury's secular perihelion (·29743) + Mars secular aphelion $(1\cdot73633)=2\cdot03376$; $2\cdot03376\div2=1\cdot01688$; Earth's present aphelion $=1\cdot01678$. . . . . (6)

The nucleo-atmospheric relations of Earth and Jupiter (3) are further shown by the fact that a nucleal expansion of Sun to Earth's secular perihelion would involve an atmospheric expansion to $(200\cdot307^{\frac{4}{3}}=)1172$ solar radii. Jupiter's mean aphelion $= 1166\cdot61$ solar radii . . . . (7)

The present light-modulus (log $M\div\odot r=5\cdot675554$) : Earth's accelerative radius (log $2\rho^2 = 4\cdot965340$) :: Jupiter's secular aphelion (log $=\cdot741881$) : Earth's secular aphelion (log $=$ ·028463). . . . . (8)

Earth's rotating, relatively to its orbital, velocity has been accelerated $366\cdot256$ times since its theoretical nebular rupture. This represents the ratio of Earth's nucleal rupturing- to Sun's aggregating-radius (2). For if we let $\rho =$ superficial radius and velocity of perfect fluidity in the æthereal nucleus (1), $\rho\sqrt2 =$ radius of dissociating velocity, and $2\rho^2 =$ radius of rupturing *vis viva* ; log $\rho(= \oplus$ secular perihelion$)=2\cdot301695$, log $(2\rho^2\div219\cdot0894)=2\cdot563791 = $ log $366\cdot253$. . . . (9)

The increased acceleration of Jupiter's angular velocity, relatively to its nucleal companion Earth, is such as would be due to the difference of orbital velocities at the outer and inner edges of the Jovi-Telluric belt. Log ♃ secular aphelion $(\cdot741881) - $ log ⊕ secular perihelion $(\bar{1}\cdot969540)= $ log $2\cdot433^2$. 24 h. $\div2\cdot433=9$ h. 51 m. 49 s. . . . . (10)

Jupiter's rotating, relatively to its orbital, velocity has been accelerated in the ratio of its mean rupturing- to Sun's aggregating-radius. For $\log \rho$ ($= \, \mathrm{24}$ mean perihelion)$= 3\cdot029231$; $\log (2\rho^2 \div 219\cdot0894) = 4\cdot018865 = \log 10443\cdot97$; $4332\cdot585$ d. $\div 10443\cdot97 = 9$ h. 57 m. 22 s.   See (7), (35), (48). . (11)

Saturn's rotating, relatively to Jupiter's orbital, velocity has been accelerated in the ratio of Jupiter's nucleal rupturing- to Sun's aggregating-radius. For $\log \rho$ ($= \, \mathrm{24}$ sec. per.)$= 3\cdot021137$; $\log (2\rho^2 \div 219\cdot0894) = 4\cdot022677 = \log 10061\cdot83$; $4332\cdot585$ d. $\div 10061\cdot83 = 10$ h. 20 m. 3 s. . . . . (12)

Saturn's rotating, relatively to its orbital, velocity has been accelerated in the ratio of Saturn's initial rupturing-radius to Earth's radius of rupture. For $\log \rho$ ($= \, \mathrm{h}$ sec. aph.)$= 3\cdot346812$; $\log (2\rho^2 \div 200\cdot307) = 4\cdot391929 = \log 24656\cdot36$; $10759\cdot22$ d. $\div 24656\cdot36 = 10$ h. 14 m. 4 s. . . . . (13)

The rotating velocity of Mars, relatively to its orbital velocity, has been accelerated nearly in the ratio of its nucleal rupturing-radius to Earth's secular aphelion. For $\log \rho$ ($= \, \mathrm{♂}$ sec. per.) $= 2\cdot449775$; $\log (2\rho^2 \div 229\cdot413) = 2\cdot839962 = \log 691\cdot77$; $686\cdot98$ d. $\div 691\cdot77 = 23$ h. 49 m. 49 s. - . . (14)

The rotating velocity of Venus, relatively to its orbital velocity, has been accelerated in the ratio of its mean rupturing-radius to Earth's mean perihelion. For $\log \rho = 2\cdot191493$; $\log (2\rho^2 \div 207\cdot583) = 2\cdot366824 = \log 232\cdot715$; $224\cdot7$ d. $\div 232\cdot715 = 23$ h. 13 m. 36 s. . . . . (15)

The rotating velocity of Mercury, relatively to its orbital velocity, has been accelerated in the ratio of its initial rupturing-radius to Sun's aggregating-radius. For $\log$ ($\rho = \, \mathrm{☿}$ sec. aph.) $= 1\cdot990608$; $\log (2\rho^2 \div 219\cdot0894) = 1\cdot941619 = \log 87\cdot422$; $87\cdot97$ d. $\div 87\cdot422 = 24$ h. 9 m. 2 s. . . . . (16)

Jupiter's secular aphelion ($5\cdot5193$) is a mean proportional between Earth's mean distance and Neptune's secular aphelion ($30\cdot4696$).   See (27) to (29). . . . . (17)

The secular perihelion of Uranus ($17\cdot688$) is at the centre of the supra-asteroidal belt.   For Neptune's secular aphelion ($30\cdot47$) + Jupiter's secular perihelion ($4\cdot886$) $= 35\cdot356$; $35\cdot356 \div 2 = 17\cdot678$. . . . . (18)

The secular perihelion of Uranus, or its locus of nebular rupture, is also a mean proportional between Saturn's secular aphelion ($10\cdot343$) and Neptune's mean aphelion ($30\cdot336$). (19)

The centres of the outer and inner planetary belts are so related that the mean distance of Uranus ($19\cdot184$) and Earth's rupturing-locus (sec. per. $= \cdot932$) are at apsides of a major axis

which would be traversed by light-undulations in the time of planetary revolution at Sun's surface. For $19 \cdot 184 + \cdot 932 = 20 \cdot 116$; $688 \cdot 3 \times 2\pi \div 214 \cdot 86 = 20 \cdot 128$. . . . . . (20)

The major axis of the November meteoric orbit is also nearly equivalent to the major axis of these primeval light-undulations. For the meteoric period $= 33 \cdot 25$ yrs.; $2 \times 33 \cdot 25^{\frac{2}{3}} = 20 \cdot 68$. . . . . (21)

When Sun's surface of dissociation was at the extremity of Earth's mean radius vector, the locus of complete aggregation, or the vertex of the stellar-solar paraboloid[*], was at Mercury's present perihelion ($\cdot 3187$). For $1 \div \pi = \cdot 3184$. . . (22)

The orbital velocity varies as the $\frac{1}{4}$ power of the gravitating velocity (3). The orbital velocity at the mean aphelion of the intra-asteroidal belt is equivalent to the mean velocity of the centripetal gravitating impulses beyond the belt. For log (sec. aph. $\Psi \times$ sec. aph. δ)$^{\frac{1}{8}} = \cdot 215437$; log mean aph. $\delta = \cdot 215944$. . . . . (23)

The mean velocity of the centripetal gravitating impulses in the principal nucleal belt is also equivalent to the mean-aphelion intra-asteroidal velocity. For log (sec. aph. $\hbar \times$ mean $\mathougate{2\!\!\!\!\!_4}$)$^{\frac{1}{8}} = \cdot 216362$. . . . . (24)

There is therefore an equivalence between the mean exterior and the mean nucleal gravitating impulses beyond the Telluric belt. For log (sec. per. $\Psi \times$ sec. aph. δ)$^{\frac{1}{2}} = \cdot 855866$; log (sec. aph. $\hbar \times$ mean per. $\mathougate{2\!\!\!\!\!_4}$)$^{\frac{1}{2}} = \cdot 855450$. . . . . . . (25)

The orbital velocity varies as the $\frac{1}{2}$ power of the rotating velocity of a varying nebula. The mean orbital velocity due to nebular action in the Neptuno-Uranian belt is equivalent to the rotating velocity at the locus of nebular rupture in the principal nucleal belt. For log (mean per. $\Psi \times$ mean δ)$^{\frac{1}{4}} = \cdot 689039$; log sec. per. $\mathougate{2\!\!\!\!\!_4} = \cdot 688982$. . . . . . . (26)

The initial rupturing-position of the centre of planetary mass (17) is determined by the mean influence of the intra-asteroidal centre (6), the supra-asteroidal centre (18), and the nebular centre of planetary inertia. For log (mean $\oplus \times$ sec. per. $\delta \times$ mean $\hbar$)$^{\frac{1}{3}} = \cdot 742338$; log sec. per $\mathougate{2\!\!\!\!\!_4} = \cdot 741881$. (27)

The same position is also a mean proportional between the centre of the supra-asteroidal and the outer limit of the intra-

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[*] It was inadvertently stated in the article on "Correlations," that there are nine abscissæ between Neptune and α Centauri. There are nine in my original paraboloid; but if the vertex is taken at the locus of complete solar aggregation, there are eighteen.

asteroidal belt.   For log (sec. per. ⚷ × sec. aph. ♂)$^{\frac{1}{2}}$ = ·743575.   .   .   .   .   (28)

The nebula-rupturing position of the centre of planetary mass is at the centre of the initial planetary system.   For sec. aph. ♆ (30·470) − sec. aph. ⚷ (20·679) = 2 × sec. per. ♃ (4·886).   .   .   .   .   (29)

The initial position of mean planetary inertia is determined by the mean positions of the rupturing-loci of the two principal two-planet belts.   For log (⚷ × ♃)$^{\frac{1}{2}}$ = ·999583 ; log mean aph. ♄ = 1·000003.   .   .   .   .   (30)

The atmospheric limit (4) of the infra-asteroidal belt is determined by positions of Sun, Jupiter, and Neptune.   For log ($\overline{♃ × ♆^{\frac{1}{2}} ÷ ☉ r}$) = 3·429079 ;   log (sec. aph. ♂ ÷ ☉ r)$^{\frac{4}{3}}$ = 3·429048.   .   .   .   .   (31)

The atmospheric limit of the initial position of the infra-asteroidal centre is determined by positions of Sun, Jupiter, and Saturn.   For log ($\overline{\text{sec. per. } ♃ × \text{sec. per. } ♄}^{\frac{1}{2}} ÷ ☉ r$) = 3·147264 ; log (sec. aph. ⊕ ÷ ☉ r)$^{\frac{4}{3}}$ = 3·147491.   .   .   .   (32)

The atmospheric limit of the initial tendency to infra-asteroidal rupture is determined by positions of Sun, Jupiter, and Earth.   For log (mean per. ♃ × ⊕)$^{\frac{1}{2}}$ = 2·680693 ; log (sec. aph. ☿ ÷ ☉ r)$^{\frac{4}{3}}$ = 2·680615.   .   .   .   .   (33)

The atmospheric limit at the inner locus of infra-asteroidal rupture is the nucleal rupturing-limit of Mars relatively to Earth.   For log (sec. per. ☿ ÷ ☉ r)$^{\frac{4}{3}}$ = 2·420721 = log 1·226 ⊕ r. vec.;   (sec. per. ♂ ÷ ⊕)$^{\frac{3}{4}}$ = 1·225.   .   .   .   .   .   .   (34)

The atmospheric limit at the central locus of infra-asteroidal rupture is at Jupiter's mean aphelion.   For log (sec. per. ⊕ ÷ ☉ r)$^{\frac{4}{3}}$ = 3·068927 ; log (mean aph. ♃ ÷ ☉ r) = 3·066743. (35)

The atmospheric limit at the rupturing-locus of Mars is near the rupturing-limit of Saturn.   For log (sec. per. ♂ ÷ ☉ r)$^{\frac{4}{3}}$ = 3·266367 ;   log sec. per. ♄ ÷ ☉ r = 3·273391 ;   3·273391 − 3·266367 = ·007024 = log 1·0163.   This indicates a similarity of contraction at the centre (6) and at the outer limit of the belt. .   .   .   .   (36)

The atmospheric limits of the Venus belt, as determined by reference to the rupturing-position of Mercury, are in or near the Earth belt.   For log (♀ ÷ sec. per. ☿)$^{\frac{4}{3}}$ ÷ ⊕ r. vec. = $\overline{1}$·942238 @ ·024175 ; log ⊕ = $\overline{1}$·969540 @ ·028463.   .   .   (37)

The atmospheric limits of the Earth belt, referred to the rup-

turing-position of Mercury, are within the Mars belt. For log $(\oplus \div \text{sec. per. } \varphi)^{\frac{4}{3}} = \cdot131591 @ \cdot210155$; $\log \delta = \cdot117620 @ \cdot239631.$ . . . . (38)

The atmospheric limits of the Mars belt, referred to the rupturing-position of Mercury, are within the asteroidal belt. (39)

The atmospheric limit at Venus's mean aphelion, referred to Mercury's mean locus, is at Earth's rupturing-locus. For $\log (\text{mean aph. } \varphi' \div \varphi)^{\frac{4}{3}} = \cdot382120$; $\log (\text{sec. per. } \oplus \div \varphi) = \cdot381719.$ . . . . (40)

The atmospheric limit at Earth's initial locus, referred to the initial locus of Mercury, is at the mean perihelion of Mars. For $\log (\text{sec. aph. } \oplus \div \text{sec. aph. } \varphi)^{\frac{4}{3}} = \cdot466876$; $\log (\text{mean per. } \delta \div \text{sec. aph. } \varphi) = \cdot468819.$ . . . . (41)

The initial locus of Earth is at the mean aphelion thermal radius of Venus. For log $1\cdot4232$ (mean aph. $\varphi \div \oplus$) $= \cdot027677$; log sec. aph. $\oplus = \cdot028463.$ . . . . (42)

The atmospheric limit at the rupturing-locus of Mars (36), referred to the rupturing-locus of Venus, is near the mean aphelion of Mars. For log (sec. per. $\delta \div$ sec. per. $\varphi)^{\frac{4}{3}} = \cdot214318$; log mean aph. $\delta = \cdot215944.$ . . . . (43)

The inner atmospheric limit of the Jupiter belt, referred to the rupturing-locus of Venus, is at Saturn's mean distance. For log (sec. per. $\mathcal{U} \div$ sec. per. $\varphi)^{\frac{4}{3}} = \cdot976134$; log $\hbar = \cdot979496.$ . . . . (44)

The outer atmospheric or initial limit of the Jupiter belt, referred to the rupturing-locus of Venus, is near Saturn's initial locus. For log (sec. aph. $\mathcal{U} \div$ sec. per. $\varphi)^{\frac{4}{3}} = 1\cdot046666$; log sec. aph. $\hbar = 1\cdot014657$; $1\cdot046666 - 1\cdot014657 = \cdot032009 = \log 1\cdot077 = \log (\text{sec. aph. } \div \text{mean } \delta)$. See (3) to (5), (20). . . . . (45)

The inner atmospheric limit of the Saturn belt, referred to the rupturing-locus of Venus, is at the initial locus of Uranus. For log (sec. per. $\hbar \div$ sec. per. $\varphi)^{\frac{4}{3}} = 1\cdot312473$; log sec. aph. $\delta = 1\cdot315531.$ . . . . (46)

The outer atmospheric limit of the Saturn belt, referred to the rupturing-locus of Mars, is also at the initial locus of Uranus. For log (sec. aph. $\hbar \div$ sec. per. $\delta)^{\frac{4}{3}} = 1\cdot313669.$ (47)

The inner atmospheric limit of the Jupiter belt, referred to Earth's rupturing-locus, is near Saturn's rupturing-locus. For log sec. per. $\mathcal{U} \div$ sec. per. $\oplus)^{\frac{4}{3}} = \cdot928796$; log. sec. per. $\hbar = \cdot941236$; $\cdot941236 - \cdot928796 = \cdot012440 = \log 1\cdot029.$. (48)

The outer atmospheric limit of the Jupiter belt, referred to Earth's rupturing-locus, is at the mean aphelion of Saturn. For log (sec. aph. $\mathrm{2\!\!\!l} \div$ sec. per. $\oplus$)$^{\frac{4}{3}} = \cdot999328$; log mean aph. $\hbar = 1\cdot000003.$ . . . . (49)

The mean atmospheric limit of the Saturn belt, referred to Earth's mean locus, is near the mean aphelion of Uranus. For log ($\hbar \div \oplus$)$^{\frac{4}{3}} = 1\cdot305995$; log mean aph. $\delta = 1\cdot301989.$ (50)

The atmospheric limit at Jupiter's mean aphelion, referred to the rupturing-locus of Mars, is at Saturn's rupturing-locus. For log (mean aph. $\mathrm{2\!\!\!l} \div$ sec. per. δ)$^{\frac{4}{3}} = \cdot940244$; log sec. per. $\hbar = \cdot941236.$ . . . . (51)

The mean atmospheric limit of the Uranus belt, referred to Jupiter's rupturing-locus, is at Neptune's mean aphelion. For log ($\delta \div$ sec. per. $\mathrm{2\!\!\!l}$)$^{\frac{4}{3}} = 1\cdot480913$; log mean aph. $\Psi = 1\cdot481951.$ . . . . (52)

The same limit (52), referred to Jupiter's mean perihelion, is at Neptune's mean locus. For log ($\delta \div$ mean per. $\mathrm{2\!\!\!l}$)$^{\frac{4}{3}} = 1\cdot478215$; log $\Psi = 1\cdot477611.$ . . . . (53)

The same limit, referred to Jupiter's mean position, is at Neptune's rupturing-locus.   For log ($\delta \div \mathrm{2\!\!\!l}$)$^{\frac{4}{3}} = 1\cdot471828$; log sec. per. $\Psi = 1\cdot471268.$ . . . . (54)

The important influence of Earth's position at a centre of early nucleal condensation is also shown by the simplicity of relations between Earth's radius vector and the secular epicyclical undulations of the supra-asteroidal planets.

Earth and Sun are convertible points of suspension for a linear pendulum equivalent to the secular excursion of Uranus. For $3 \div 38\cdot365 = \cdot0782$; the maximum eccentricity of Uranus is $\cdot0780$.   See (20), (21). . . . . (55)

The excursion of Saturn is nearly equivalent to the atmospheric limit of a nucleus which has Earth's thermal radius ($1\cdot4232^{\frac{4}{3}} = 1\cdot601$). For $1\cdot601 \div 19\cdot078 = \cdot0839$; the maximum eccentricity of Saturn is $\cdot0843$.   . . . . (56)

The excursion of Jupiter is equivalent to the mean radius of rotating inertia at Earth's orbit ($\sqrt{\cdot4} = \cdot6325$).   For $\cdot6325 \div 10\cdot406 = \cdot06078$, Jupiter's maximum eccentricity being $\cdot06083$.   . . . . (57)

The excursion of Neptune is in the inverse ratio of its own coefficient ($\frac{8}{7}$), and in the direct ratio of the coefficient of Uranus ($\frac{7}{8}$), in the abscissas of the solar aggregating-paraboloid.   For $\frac{7}{8} \div 60\cdot074 = \cdot0146$; Neptune's maximum eccentricity is $\cdot0145$.   . . . . (58)

The following Table shows the closeness of approximation, (theoretical − observed) ÷ observed in each of the foregoing comparisons:—

1.	−·0039	16.	−·0028 *	31.	·0001	45.	−·0009
2.	·0197	17.	·0001	32.	−·0005	46.	−·0070
3.	−·0158	18.	−·0006	33.	·0002	47.	−·0043
4.	·0109	19.	·0019	34.	·0008	48.	−·0291
5.	−·0014	20.	·0006	35.	·0050	49.	−·0016
6.	·0001	21.	−·0224	36.	·0006	50.	·0093
7.	·0033	22.	−·0013	37.	·0000	51.	−·0023
8.	·0074	23.	−·0012	38.	·0000	52.	−·0025
9.	·0000	24.	·0010	39.	·0000	53.	−·0014
10.	·0070*	25.	·0010	40.	·0009	54.	−·0013
11.	−·0023*	26.	·0001	41.	−·0045	55.	·0030
12.	·0169*	27.	·0011	42.	−·0018	56.	−·0050
13.	·0238*	28.	·0039	43.	−·0038	57.	−·0008
14.	·0319*	29.	·0019	44.	−·0078	58.	·0014
15.	·0052*	30.	−·0010				

XXVIII. *On the Speed of Signalling through Heterogeneous Telegraph Circuits.* By Oliver Heaviside[†].

WHEN the first trials of speed of working were made on the Anglo-Danish cable, then recently laid (September 1868), it was found that a considerably higher speed could be reached in one direction than in the other. The " line " portion of the circuit consisted of a land-line on the English side of 240 ohms resistance, then a cable of 2500 ohms resistance and capacity 120 microfarads, and a land-line on the Danish side of 1250 ohms—all approximate. The circuit was completed through a battery of 150 ohms at one end and a Wheatstone's receiver of 750 ohms at the other, the circuit being worked on the earth-to-earth principle, *i. e.* without condensers. But although the battery and receiver at each end were the same, or nearly so, the maximum speed obtained with Wheatstone's transmitter, making mechanically exact signals, was 40 per cent. higher from England to Denmark than from Denmark to England[‡]. This unexpected result was abundantly confirmed by the subsequent experience of every-day practice, which proved the existence of a difference in working-speed in opposite directions varying from 20 to 40 per cent. at different times, mainly according to the state of insulation of the land-lines.

[*] According to Herschel.
[†] Communicated by the Author.
[‡] It may be interesting to state the actual speeds obtained on this circuit with different instruments. Morse, 60 to 75 letters per minute; Wheatstone's transmitter and receiver, 90 to 140 letters per minute; Wheatstone's transmitter and Thomson's recorder, 300 to 360 letters per minute : in all cases without condensers.

Later on the same instruments were introduced between London and Amsterdam, on a circuit consisting of a land-line of 130 miles on the English side, then a cable of 120 miles, and on the Dutch side a land-line of 20 miles (Culley, Journ. Soc. Tel. Eng. vol. i.). In this case the maximum speed obtained was 50 per cent. higher from Amsterdam to London than *vice versâ*. Again, on the London-Dublin circuit, consisting of cable 66 miles and land-lines 266 and 10 miles, the longer line being on the English side, the speed from Dublin to London was double that obtained in the reverse direction, viz. 80 and 40 words per minute respectively. Similarly between London and Belfast.

In all these cases it is to be observed that the station nearest the cable receives the most slowly, and that the greater the inequality of resistance of the land-lines, the greater is the difference in the working-speeds. This seems to point directly to the conclusion that the uncentrical position of the cable in the circuit actually causes the retardation to be greater in one direction than in the other. The fact that the cable receives a much larger charge of electricity when the battery is connected to the end of the shorter than to the end of the longer land-line might, on a cursory examination, seem to corroborate this conclusion. But when the light of theory is thrown upon this view of the matter it is at once found to be untenable.

It is easily shown that if condensers be distributed in any arbitrary manner along a line which is to earth at each end, dividing it into sections having any resistances, and the condensers be all initially discharged, the introduction of an electromotive force in the first section will cause the current to rise in the last section, in the same manner as the same electromotive force in the last section will cause the current to rise in the first section. Furthermore, it may be shown that if leaks be introduced on the line in any arbitrary manner, the same property will hold good. (The differential equation of the current, which is linear and of the same degree as the number of condensers, is the same for the first and last sections; and the conditions to determine the arbitrary constants are the same.) Now every telegraph-line, however irregular it may be in its resistance, capacity, and insulation in different places, may be considered as such a system of condensers and leaks, infinite in number if necessary; whence it follows that on any line there is absolutely no difference in the retardation in either direction, meaning by *retardation* the time required for an electromotive force at one end to cause the current at the other end to reach any stated fraction of its maximum. Therefore, to account for the facts, which cannot be gainsaid, we must

look outside the line and fix our attention on the sending- and receiving-apparatus. The actual cause or causes must, however, be of such a nature that they only come into operation when the capacity, or the leakage, is unsymmetrically situated in the circuit. No perceptible difference in working-speed was observed on the Anglo-Danish circuit when the correspondence was maintained between the two ends of the cable itself. Now, since in all the cases described Wheatstone's transmitter was employed, it is natural to inquire whether the difference is due to any peculiarity in the method of making the signals with that instrument. If so, then we need not expect any difference to exist when simple reversals are made. But, in fact, it exists even then. An instance bearing this out was described by Mr. Varley before the Submarine-Cable Committee (Sub. Report, p. 156). Experimenting with his "wave-bisector" on the underground lines between London and Liverpool, Mr. Varley found that the introduction of resistance at the battery-end of the line lowered the speed to a greater extent than its introduction at the receiving-end, where indeed it made little difference. Here the speed was inversely as the retardation, since the wave-bisector made simple reversals. Mr. Varley attributed the difference to the leakage; but this is in direct contradiction to the theoretical result, that neither leakage nor irregularity in distribution of capacity can, acting alone, cause any difference. Also the difference existed on the Anglo-Danish circuit when simple reversals were made with the transmitter, but apparently to a smaller extent. It was quite perceptible (10 or 20 per cent.) with key-sending, using a common reversing key—though the exact amount of the difference could then not be exactly estimated, since operators differ nearly as much in their hand-signalling as in their hand-writing. Although, therefore, in the case of Wheatstone's transmitter the difference in working-speed may be, and I believe is, mainly due to a peculiarity of that instrument, yet when plain reversals are sent, there must actually be a difference in the retardation in opposite directions; and this I believe is due to the fact, which comes out on closer inspection, that it is not the same circuit which is being worked when the direction of working is reversed.

Let the line consist of a cable of resistance c, having land-lines of resistances a and b attached to its ends, and let the battery and receiver resistances be f and g respectively. Then fig. 1 shows the arrangement when A sends to B. Further, suppose for simplicity, and to avoid analytical calculations, that the cable's resistance is small compared with the total resistance of the circuit. Then we may obtain tolerably accu-

rate results by considering the cable's capacity as collected at its centre. Then, by the theory of the condenser, when A ap-

Fig. 1.
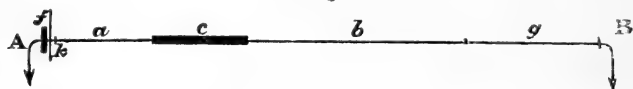
plies his battery to the line, the current rises at B according to the formula

$$C = \frac{E}{R}(1 - \epsilon^{-\frac{t}{T}}),$$

where C is the current, E the electromotive force, R the total resistance between A and B, t the time, and

$$T = \frac{S}{R}\left(\frac{c}{2} + a + f\right)\left(\frac{c}{2} + b + g\right),$$

where S is the cable's capacity. Thus the magnitude of T determines the slowness of the rise of the current, and we may therefore call it the retardation. (In the time T, the current reaches about 63 per cent. of its maximum.) Now when B sends to A, f and g change places, producing the arrangement shown in fig. 2. If C′ is the current B produces at A,

Fig. 2.

$$C' = \frac{E}{R}(1 - \epsilon^{-\frac{t}{T'}}),$$

where

$$T' = \frac{S}{R}\left(\frac{c}{2} + a + g\right)\left(\frac{c}{2} + b + f\right).$$

Comparing the values of T and T′, we shall find that if $a = b$, T = T′; also if $f = g$, T = T′; but if $a < b$, as in the figures, T < T′ if $f < g$, and T > T′ if $f > g$. Or, in plain English, the retardation is the same in both directions if the land-lines have equal resistances, whatever may be the resistances of the battery and receiver; it is also the same in both directions if the battery and receiver have equal resistances, whatever may be the resistances of the land-lines; but if the resistances of the land-lines are unequal, the retardation is greatest when the station nearest the cable is receiving, if at the same time the

battery is less than the receiver resistance, and least in the contrary case. Now if the battery is always in circuit, as in making signals with a reversing key, the effect of any arbitrary signals may be calculated by the same formula, and the maximum working-speed (always provided it be within the reach of the apparatus) will be least when the station nearest the cable receives, if the battery is less than the receiver resistance, and greatest in the contrary case. Generally, the more centrally the capacity is situated the greater the retardation.

The influence of leakage or faults may be readily determined in a similar manner, since the retardation is proportional to the resistance through which the charge in the cable discharges to earth. In all cases the retardation is reduced by a fault, and the more so the nearer the fault is to the centre of capacity. If a fault be introduced on the long land-line b, the difference of the retardation in opposite directions is the same as before as regards direction, while its percentage amount is increased. The influence of the natural leakage of the land-lines is the same, since nearly all the loss will, under ordinary circumstances, take place on the long land-line. But if a fault be introduced on the short land-line, the percentage difference is reduced instead of being increased, and its direction may even be reversed.

We have thus found that on any circuit consisting of a cable with land-lines of unequal resistance at its ends, a difference in the retardation in opposite directions is necessarily introduced when the battery and receiver have not the same resistance. Suppose, in figs. 1 and 2, $f=1$, $a=10$, $c=10$, $b=100$, $g=10$; then the retardation from A to B is to the retardation from B to A as $184:265$, *i. e.* 44 per cent. greater from B to A than from A to B; and the natural leakage of the land-lines increases this difference. But with Wheatstone's transmitter the observed difference is greater than can be thus accounted for, and exists even when there is no inequality in resistance of the battery and receiver. This is due to a peculiarity in the method of making the signals with that instrument, which is at the same time the cause of two other anomalies, viz.:—reduction of working-speed by leakage, although the retardation is thereby reduced; and increase of working-speed by the addition of resistance, although the retardation is thereby increased. To understand this, it is necessary to examine the way the sending-end of the line is operated upon. The point k in fig. 1, or k' in fig. 2, is always connected with the positive or negative pole of the battery, or it is insulated. Currents of equal duration follow each other, alternately $+$ and $-$, separated either by no interval, or by intervals equal

to twice, four, or six times the time of a current*. The arma-
ture of the receiver is adjusted neutral, so as to remain on the
side any current sends it to, until an opposite current reverses
its position.   Lines of two lengths are thus made:—a "dot"
by first a + current immediately followed by a − current
to terminate it, thus + − ; and a "dash" of three times
the length by first a + current, then an interval of insulation
for twice as long, and lastly a − current to terminate it, thus
+ 00 −.   At a speed much below the limiting speed the sent
signals are reproduced at the receiving-end without sensible
alteration ; but as the speed of working is increased and the
currents have not time to reach their full strength, irregulari-
ties show themselves, which increase rapidly as the length of
each contact is reduced, until at length a limiting speed is
reached at which some of the signals miss fire altogether.
Consider the succession of signals

$$a \quad b \quad c \quad d \ e \quad\quad f \ g \ h \quad i \ j$$
$$+ - + - + - + - 0000 + - + 00 - + - 0000$$

(illustrating a typical failure), consisting of a series of dots,
followed, after an interval of insulation, by a dot, a dash, and
a dot.   If the receiver is adjusted so as to record the dots a, b,
c, d perfectly, the signals g and j will fail. g will fail because
the − current e has time to die away during the interval of
no sent current 0000, thus making the succeeding + current
f too strong ; and j will fail because the + current h has
time to die away during the interval of no sent current 00,
thus making the − current i too strong.   In the first case the
dot is continued on to the dash, in the second the dot is lost.
Thus, although generally, to get the greatest possible working-
speed, the retardation should be as small as possible, yet in this
system of contacts of equal duration to make lines of unequal
length, it is important that *some* of the currents, viz. those com-
mencing dashes or spaces, should not die away too quickly.
They are prevented from doing so, in a great measure, by the
insulation of the line at the sending-end during the intervals of
no sent current, which, by closing up the path at one end for
the charge to escape, prolongs the current at the other.   (The
compensation currents, sent by an improved form of trans-
mitter, have for their object to still further lengthen out the
currents.)   Now it will be seen from the figures that when A
insulates the line at k, fig. 1, the charge of the cable discharges
through the resistance $\frac{c}{2} + b + g$, and that when B insulates
at k', fig. 2, it discharges through the smaller resistance

* Mr. Culley's 'Handbook' contains a full description of the apparatus.

$\frac{c}{2} + a + g$.   Therefore the current dies away more quickly in the latter case, and, by reason of the before-mentioned peculiarities, the station A nearest the cable receives more slowly than B.   The explanation of the reduction of speed by leakage is similar.   The leakage lessens the retardation and consequently quickens the signals.   If every signal were quickened in the same proportion, as would happen were the circuit always complete, it is evident that the speed of working must be increased ; but it is easily seen that the decrease in the retardation caused by the loss is proportionally much less when the circuit is complete than when the line is insulated at the sending-end, thus increasing the irregularity in the received signals due to the unequal intervals between the sent signals, and consequently lowering the working-speed. Again, the addition of resistance at the receiving-end, as at A in fig. 2, when B sends to A, may increase the working-speed.   Now, since the addition of resistance obviously increases the retardation, nothing could result save a decrease of speed if the retardation of every signal were increased in the same ratio.   But this is not the case ; for the retardation is increased in a greater ratio when the line is insulated at the sending-end than when the circuit is complete —exactly the opposite to what occurs with leakage: then the working-speed was lowered ; now it is increased.   (This reasoning will not, of course, apply to other systems of transmission.)   On the other hand, the speed is lowered by inserting resistance at the sending-end, B, fig. 2 ; for the retardation is unaltered with line insulated, and increased with complete circuit.

To ascertain the exact amount of retardation produced by resistance at either or both ends of a submarine cable, each case must be calculated separately, because the form of the curve of arrival of the current is altered, the law of the squares only holding good when exactly similar systems are compared.

A   B   C   D

Let B C be a cable of length l, resistance k per unit of length, capacity c per unit of length ; and let A B and C D be resistances equal to mkl and nkl respectively, connected to the cable at B and C, and to earth at A and D.   Let v be the potential of the conductor of the cable at distance x from B at the time t.   Then, according to Sir W. Thomson's theory, v must satisfy

$$\frac{d^2v}{dx^2} = ck\frac{dv}{dt}$$

between $x=0$ and $x=l$. The general solution is

$$v=\Sigma \mathrm{A} \sin \left(\frac{ax}{l}+b\right)\epsilon^{-\frac{a^2t}{\mathrm{T}}}, \quad . \quad . \quad . \quad (1)$$

where $\mathrm{T}=ckl^2$, if v vanishes for $t=\infty$. Three sets of constants, A, a, and b, have to be determined from the terminal conditions for x and t. In A B and C D the current follows Ohm's law. Therefore

$$-\frac{v}{mkl}=-\frac{1}{k}\frac{dv}{dx} \text{ when } x=0,$$

and

$$\frac{v}{nkl}=-\frac{1}{k}\frac{dv}{dx} \text{ when } x=l,$$

for all values of t. Therefore, by (1),

$$\sin b=ma \cos b, \text{ or } \tan b-ma=0,$$

and

$$\sin (a+b)=-na \cos (a+b),$$

or

$$\tan (a+b)+na=0.$$

Hence, eliminating b,

$$\tan a=\frac{(m+n)a}{mna^2-1},$$

from which the a's can be found when m and n are given. The b's are already known in terms of the a's, and the A's can be found by integration if the potential of every part of the conductor of the cable is given for $t=0$. Let it be that produced by an electromotive force E in A B, *i. e.*

$$v=\mathrm{E}\cdot\frac{l(1+n)-x}{l(1+m+n)};$$

then, by integration,

$$\mathrm{A}=\frac{\dfrac{2\mathrm{E}\cos b}{a}}{1+\dfrac{m}{1+m^2a^2}+\dfrac{n}{1+n^2a^2}};$$

and finally, the potential at time t is

$$v=\sum_{i=1}^{i=\infty}\frac{2\mathrm{E}}{a_i}\frac{\dfrac{1}{1+m^2a_i^2}}{1+\dfrac{m}{1+m^2a_i^2}+\dfrac{n}{1+n^2a_i^2}}(\sin +ma_i\cos)\frac{a_ix}{l}\epsilon^{-\frac{a_i^2t}{\mathrm{T}}},$$

from which the arrival-curves of the current may be found by making $x=l$. In the diagram six cases are shown. The
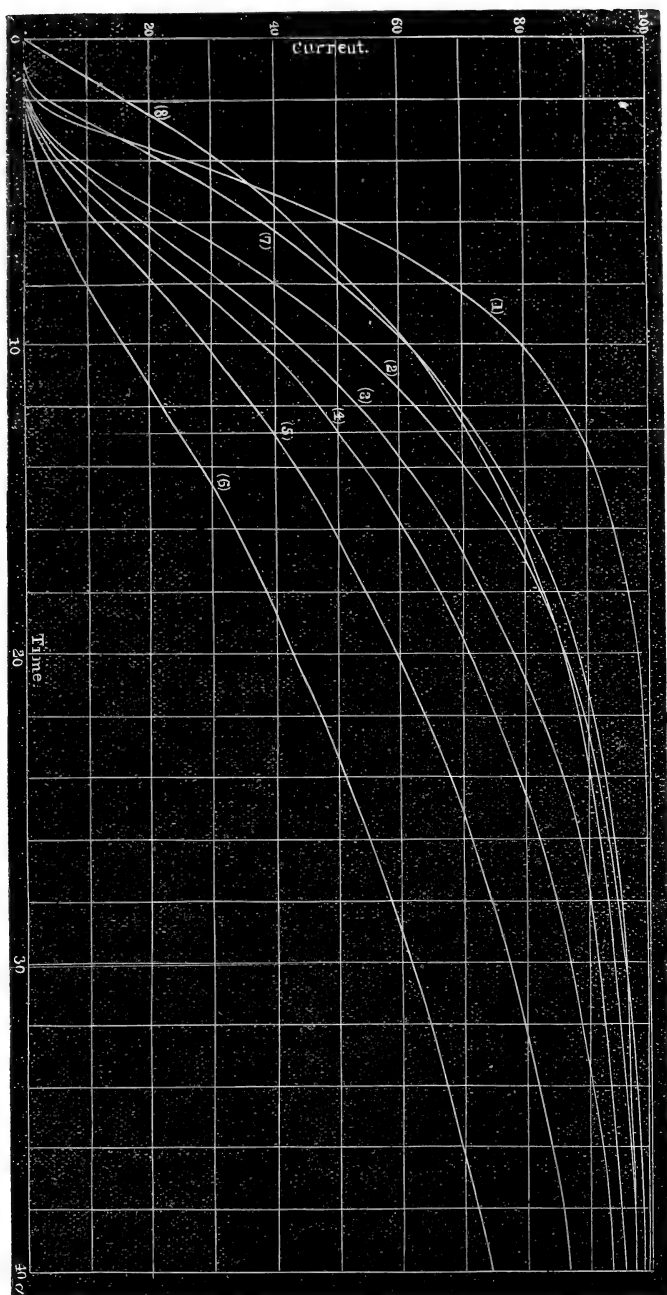
Fig. 3.

abscissas represent time, from $t=0$ to $t=40\alpha$, the unit being $\alpha = \dfrac{ckl^2}{10\pi^2} \log_e 10$. The ordinates represent the arrived current, the maximum strength being in all cases $=100$.

1. $m=0$, $n=0$. Let N be the percentage amount of received current at time t, then

$$\frac{N}{100} = 1 + \frac{2}{\pi} \Sigma \cos i\pi\epsilon^{-\frac{i^2\pi^2 t}{T}}.$$

2. $m=0$, $n=\frac{1}{2}$.

$$\tan a + \frac{a}{2} = 0,$$

$$\frac{N}{100} = 1 + \Sigma \frac{12 \cos a}{5 + \cos 2a} \epsilon^{-\frac{a^2 t}{T}}.$$

3. $m=0$, $n=1$.

$$\tan a + a = 0,$$

$$\frac{N}{100} = 1 - 4\Sigma \frac{\epsilon^{-\frac{a^2 t}{T}}}{\cos a + \sec a}.$$

4. $m=0$, $n=2$.

$$\tan a + 2a = 0,$$

$$\frac{N}{100} = 1 - \Sigma \frac{6 \cos a}{2 + \cos a} \epsilon^{-\frac{a^2 t}{T}}.$$

5. $m=0$, $n=\infty$.

$$\frac{N}{100} = 1 + \frac{4}{\pi} \Sigma \frac{\cos i\pi}{2i-1} \epsilon^{-\frac{(2i-1)^2\pi^2 t}{4ckl^2}}.$$

6. $m=1$, $n=1$.

$$\tan a = \frac{2a}{a^2-1},$$

$$\frac{N}{100} = 1 - \Sigma \frac{6\epsilon^{-\frac{a^2 t}{T}}}{3+a^2}.$$

Curve (1) is the arrival-curve when no resistance is inserted at either end ; curve (2) when a resistance equal to one half the cable's resistance is inserted at either end ; curve (3) when a resistance equal to the cable's is inserted at either end ; and curve (4) when twice the cable's resistance is inserted at either end. (5) shows the curve of arrival of the potential at the insulated end of a cable when the other end is raised to a con-

stant potential ; (6) shows the arrival-curve when a resistance equal to the cable's is inserted at each end.

It will be observed from an inspection of the curves, that, when resistance is added at one end of a cable only, the effect in increasing the retardation is very great when the added resistance is small, but as more and more resistance is added there is not much further effect. The limit is reached in curve (5). But the insertion of resistance at both ends has a much greater retarding influence, which increases without limit. Compare (4) with (6): in (4) we have twice the cable's resistance at one end and none at the other ; in (6) the same resistance is equally divided at each end, and the retardation is very greatly increased.

With respect to the change in the form of the arrival-curves, it will be seen that, when resistance is inserted, the first part of the arrived current is proportionally less retarded than the later parts. Thus, comparing (1) with (6), when there is no resistance inserted the current reaches 5 per cent. of its maximum in $2 \cdot 45\,\alpha$, whereas (6) takes $6\,\alpha$, or $2 \cdot 4$ times as long ; to reach 10 per cent. (6) takes $3 \cdot 3$ times as long as (1) ; to reach 40 per cent. it takes $3 \cdot 7$ times as long, and to reach 70 per cent. $4 \cdot 5$ times as long.

Curves (1), (7), and (8) show the effect of different distributions of the same amount of capacity in a line of given resistance. (8) shows the arrival-curve when the capacity is all collected at the centre of the line as a single condenser, (7) when the capacity is uniformly distributed over the middle third of the line, and (1) when it is uniformly distributed over the whole length. The more the capacity is spread, the longer is the time taken for the current to reach a sensible strength, whereas the current rises rapidly the moment contact is made when the capacity is collected at one place. Curve (7) is the same as (6) with the abscissas of the latter reduced in the ratio $3 : 1$; and curve (8) is the limiting form of the arrival-curve when very great equal resistances are inserted at both ends of the cable, the abscissas being reduced in the same proportion as the resistance of the circuit is increased. Its equation is

$$\frac{N}{100} = 1 - \epsilon^{-\frac{4t}{T}}.$$

XXIX. *On Fixed Lines in the Ultra-red Region of the Spectrum.*
By Captain ABNEY, *R.E., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the February Number of the Philosophical Magazine there is a communication from Dr. Draper in which reference is made to myself. May I ask that you will insert a few remarks on the points in which I am interested?

May I premise by saying that I am excessively sorry if any paper of mine has caused Dr. Draper to think that I have depreciated his method of photographing the least-refracted end of the spectrum. I am very familiar with the method he has indicated in his paper, and have employed it with marked success. The object with which my experiments were undertaken was to find some method by which the lines in the red and ultra-red end of the spectrum could be photographed, in a manner known as *unreversed*; that is, that a black line in the spectrum should, after being photographed by the collodion process on glass, show as a transparent line in the picture when viewed by transmitted light. It is fairly easy to obtain a picture in which they appear opaque compared with the adjoining portions of the developed image; but it has hitherto proved much more difficult to obtain them as indicated above. In this attempt I have been fairly successful, and in some negatives have obtained transparent lines far below those already photographed.

It may interest Dr. Draper to know that last autumn I studied, with most encouraging results, the "antagonism" which seems to exist between various portions of the spectrum when they are used to excite a photographically sensitive plate. This research I hope to continue when the sun appears a little more frequently than it has during the last four months.

I trust I may be allowed to differ from Dr. Draper regarding the advisability of abandoning the researches previously alluded to. It seems to me that a method of obtaining a *negative* picture of the least-refrangible portion of the spectrum will be valuable in more ways than one.

Yours faithfully,
W. DE W. ABNEY.

XXX. *Notices respecting New Books.*

An Elementary Treatise on Elliptic Functions. By ARTHUR CAY-
LEY, *Sadlerian Professor of Pure Mathematics in the University of
Cambridge.* Cambridge: Deighton, Bell, and Co. London:
Bell and Sons. 1876. 8vo. Pp. 384.

IN most works on the Calculus the subject of Elliptic Integrals is
either altogether excluded or treated inadequately; and not
merely is this the case with works that are in fact elementary, but
even in the elaborate treatise of the late Professor De Morgan the
subject is dismissed in two paragraphs, which are in substance as
follows:—" Important as Elliptic Integrals are in certain classes
of problems, and numerous as have been the properties of them,
which have been investigated, it cannot yet be said that either these
problems or methods lie so close to the grand route on which the
students' elementary course should be marked out as to require a
detailed treatise on them to be inserted here." He then goes on
to state:—that an Integral is called Elliptic when it can be put into
the form $\int \dfrac{\mathrm{R}dx}{\sqrt{\mathrm{X}}}$, where R is a rational function of x, and X a
rational and integral function of the fourth degree; that it is capable
of being shown that the actual calculation of all such Integrals is
attainable when Tables of the following integrals (called elliptic
integrals of the first, second, and third kind respectively) have been
constructed, viz.

$$\int_0^\phi \frac{d\phi}{\sqrt{(1-k^2\sin^2\phi)}}, \quad \int_0^\phi \sqrt{(1-k^2\sin^2\phi)}\,d\phi,$$

$$\int_0^\phi \frac{1}{1+n\sin^2\phi} \cdot \frac{d\phi}{\sqrt{(1-k^2\sin^2\phi)}},$$

in which k (the modulus) is less than unity, and ϕ (the amplitude)
does not exceed $\frac{1}{2}\pi$; and that extensive Tables of the first two kinds
have been given by Legendre, with methods of approximating to
integrals of the third kind (pp. 656, 657). In fact Legendre worked
at the subject, systematizing and supplementing the work of his
predecessors and making the actual numerical calculations, for
about forty years. His results, in their final form, were published in
1825–26. They were scarcely out when the subject was treated from
an entirely new point of view by Jacobi, whose *Fundamenta Nova*
was published in 1829, being preceded and followed by memoirs
from 1828 to 1858, and by Abel, whose memoirs appeared from 1826
to 1829.

It will be seen from this statement that the student who wishes
to make out the subject will not gain much help from the ordinary
textbooks; and indeed not much has been written (we believe) on
the subject in English, beyond the works referred to in the note to
the above-quoted passage in De Morgan's 'Differential Calculus.'
An Elementary Treatise on Elliptic Integrals was therefore much

needed; and students have reason to be thankful that the task of writing such a book has been undertaken by one in every way so competent as Professor Cayley. His Treatise "is founded upon Legendre's *Traité des Fonctions Elliptiques*, and upon Jacobi's *Fundamenta Nova* and memoirs by him in Crelle's *Journal*;" he has made "comparatively very little use of the investigations of Abel or of those of later authors." A good deal has, however, been done in treating "various points which require to be more fully discussed" than they have been by Jacobi, and particularly the theory of the Elliptic functions themselves, and not included in the *Fundamenta Nova* the "theory of the partial differential equation satisfied by the functions Θ, H, and deduced therefrom the partial differential equations satisfied by the numerators and denominators in the theories of the multiplication and transformation of the elliptic functions."

The Treatise is expressly designed for the use of students, and great care has been taken to prevent them from being lost in the wilderness of symbols to which the author introduces them. Thus the first chapter is taken up with a general outline of the subject, and the student is directed to peruse the chapter, not dwelling on it, but returning to it as he finds occasion, the object being that he may always have certain landmarks in view, and be the more able to keep in mind the mutual relations of the parts of the subject. With the same object, introductory articles of the nature of outlines are prefixed to most of the chapters. The student is also directed to confine his attention in the first instance to five specified chapters, viz. 2, 3, 4, 12, 13. We will mention briefly the contents of these chapters, both for the purpose of giving some notion of the treatment which the subject receives at Professor Cayley's hands, and as showing what he regards as a sort of first course of the subject.

The reduction of $\int \dfrac{\mathrm{R}\,dx}{\sqrt{\mathrm{X}}}$ to one of the three kinds of Elliptic Integrals is treated in chapter 12. That this can always be done the author shows both by Legendre's method of supposing X to be decomposed into two quadratic factors, and by a method of his own based on a linear transformation of the undecomposed quartic function (X). In chapter 2 he establishes the well-known fundamental relation, viz. if $\mathrm{F}(\phi)$ denotes an elliptic integral of the first kind with amplitude ϕ, and if ψ and μ are the amplitudes of two other elliptic functions of the first kind such that

$$\mathrm{F}(\phi) + \mathrm{F}(\psi) = \mathrm{F}(\mu),$$

then $\cos \mu = \cos \phi \cos \psi - \sin \phi \sin \psi \sqrt{(1 - k^2 \sin^2 \mu)}.$

Of this equation (the addition equation) seven distinct proofs are given. It is plain that this equation enables us to determine the amplitude of the function which is the sum (or difference) of any two functions of given amplitude, and hence the amplitude of the function which is twice a function of given amplitude, and then of one which is *n* times a given function. Indeed it would be possible to lay down a method of calculating the numerical value of a function

of given modulus for an assigned amplitude by deductions from this equation. When $F(\phi)$ or u is regarded as a function of its amplitude, it is found very convenient to call $\sin\phi$ the sine of the amplitude of u, and to contract these words into the notation sn u; and similarly $\cos\phi$ and $\sqrt{(1-k^2\sin^2\phi)}$ are denoted by cn u and dn u: e. g. the addition equation given above would be written

$$\text{cn}(u+v) = \text{cn } u \text{ cn } v - \text{sn } u \text{ sn } v \text{ dn }(u+v).$$

There is no difficulty in deducing an enormous number of relations between these functions, such as

$$\text{sn}(u+v)(1-k^2\text{ sn}^2 u \text{ cn}^2 v) = \text{sn } u \text{ cn } v \text{ dn } v + \text{sn } v \text{ cn } u \text{ dn } u.$$

It will be seen that this formula closely resembles the formula for the sine of the sum of two angles, which indeed it becomes if $k=0$. The fourth chapter contains the development of the relations between the functions of the amplitudes and those of the sums, differences, and multiples of the functions.

The third chapter contains:—the solution of a number of elementary questions, particularly those relating to the curves whose arcs represent the integrals; the discussion of " the march " of the functions, or their graphic representation; some of the properties of the complete functions (viz. the integrals when $\phi=\frac{1}{2}\pi$), such as the theorem enunciated by the equation

$$EF' + E'F - FF' = \tfrac{1}{2}\pi ;$$

methods of obtaining series for the complete functions when k is either small or nearly equal to unity; and the properties of the " Gudermannian," *i. e.* the amplitude of the function $F(\phi)$ when k is unity.

It has been already mentioned that a method of calculating $F(\phi)$ numerically might be based on the addition equation. Practically speaking, however, the calculation is more conveniently effected by transforming the integral with a given amplitude and modulus into another with a different amplitude and modulus, and repeating the process of transformation until an integral is found of assigned amplitude with a modulus not differing sensibly from zero. Chapter 13 is devoted to showing how this can be done, by means of Landen's theorem, for functions of the first and second kind. The chapter contains the original proof of Landen's theorem in an altered notation. A proof of the same theorem is also given in chapter 2, as a deduction from one of Jacobi's proofs of the addition equation, viz. that depending on two circles. It may just be noticed that this first course gives a view of the subject about equivalent to that attained by Legendre shortly after he had begun his labours, though many points are here worked out systematically and in a new notation (particularly all that relates to the functions sn u &c.); and some points are new, such as the proof of the double periodicity of these functions, the properties of the Gudermannian, &c.

These chapters occupy about a third part of the volume, and will not offer any serious difficulty to the student, though he will have to dwell upon them sufficiently to become accustomed to the notation, much of which will be new to him. The difficulty increases

considerably when the properties of the third class of integrals are considered, and those of the Θ functions (in terms of which the elliptic functions can be expressed) as well as the general theory of the transformation of the elliptic functions, of which Landen's theorem is in fact a particular case.

Professor Cayley has taken a great deal of pains to clear up the difficulties to be met with at the entrance of the subject. But when all has been done, the elliptic functions will probably appear to most students at first sight somewhat intangible entities. It is perhaps with a view to this circumstance that the following paragraph was written, which we venture to extract as an admirable specimen of elementary exposition:—" In further illustration " of the elliptic functions sn u, cn u, dn u, " suppose that the theory of the circular functions sine, cosine was unknown, and that we had defined Fx to be the function

$$\int_0 \frac{dx}{\sqrt{(1-x^2)}}.$$

Then taking the variables x and y to be connected by the differential equation

$$\frac{dx}{\sqrt{(1-x^2)}} + \frac{dy}{\sqrt{(1-y^2)}} = 0,$$

and supposing that z is the value of y answering to $x=0$, we have

$$\mathrm{F}x + \mathrm{F}y = \mathrm{F}z.$$

But the differential equation admits of algebraic integration: and determining in each case the constant by the condition that for $x=0$, y shall be $=z$, the algebraic integral may be expressed in two forms,

$$x \sqrt{(1-y^2)} + y \sqrt{(1-x^2)} = z,$$
$$xy - \sqrt{(1-x^2)} \sqrt{(1-y^2)} = \sqrt{(1-z^2)};$$

so that either of these equations represents the above-mentioned transcendental integral; and thus we have a circular theory precisely analogous to the elliptic theory in its original form. But here the function Fx is the inverse function $\sin^{-1} x$, and the last-mentioned two equations are the equivalents of the equation

$$\sin^{-1} x + \sin^{-1} y = \sin^{-1} z,$$

whence writing $\sin^{-1} x = u$, $\sin^{-1} y = v$, and therefore $x = \sin u$, $y = \sin v$, $z = \sin (u+v)$; and also assuming $\sqrt{(1-\sin^2 u)} = \cos u$, and therefore $\sqrt{(1-\sin^2 v)} = \cos v$, and $\sqrt{(1-\sin^2 (u+v))} = \cos (u+v)$, the equations in question become

$$\sin(u+v) = \sin u \cos v + \sin v \cos u,$$
$$\cos (u+v) = \cos u \cos v - \sin u \sin v,$$

and it is clearly convenient to use these functions sin, cos in place of F, denoting as above $\sin^{-1}$ ..... The circular theory gives rise to a numerical transcendent π, viz. $\frac{\pi}{2} = \frac{3 \cdot 14159 \ldots}{2}$ is such a quantity that $\sin \frac{\pi}{2} = 1$, $\cos \frac{\pi}{2} = 0$, $\frac{\pi}{2}$ being the smallest positive value of the argument for which the two functions have these values; and in developing the theory from the integral $\int \frac{dx}{\sqrt{(1-x^2)}}$,

$\frac{\pi}{2}$ would be the complete function defined from the equation

$$\frac{\pi}{2} = \int_0^1 \frac{dx}{\sqrt{(1-x^2)}}.$$

Moreover the circular functions are periodic, having for their common period four times this quantity, $=2\pi$; viz. we have

$$\begin{matrix}\sin \\ \cos\end{matrix} (u+2\pi) = \begin{matrix}\sin \\ \cos\end{matrix} u.$$

Corresponding to $\frac{\pi}{2}$ we have in the elliptic functions in the first instance the complete function K; viz. K is a real positive quantity defined by the equation

$$K = \int_0^{\frac{\pi}{2}} \frac{d\phi}{\sqrt{(1-k^2 \sin^2 \phi)}},$$

where K is of course not a mere numerical transcendent, but a function of k: K is such that we have sn K$=1$, cn K$=0$, dn K$=k'$ [*i. e.* $\sqrt{(1-k^2)}$. . . . . . , and it ultimately appears that the sn, cn, and dn of $u+4$K are the same as the sn, cn, and dn of u respectively; or the functions have a real period 4K " (pp. 10, 11, 12). Our limits do not allow of our extracting the whole article; but we may just add that, as well as the analogies between the circular and elliptic functions, attention is drawn to the transformation theory, and the second period $4(K + \sqrt{-1}.K')$ in the elliptic functions, which have no analogues in the circular theory.

An Elementary Treatise on the Differential Calculus, containing the Theory of Plane Curves, with numerous examples. By BENJAMIN WILLIAMSON, *M.A., Fellow and Tutor, Trinity College, Dublin. Third Edition, revised and enlarged.* London: Longmans, Green, and Co. 1877. (Crown 8vo. Pp. 416.)

We have already noticed the first and second editions of this excellent work, and have now to notice the third. As might be expected, the work in its present form is not substantially different from what it was when it first appeared. In fact, a considerable part of the second and third editions agree page for page; so that the amount of difference may be pretty accurately inferred from the fact that the former edition contains 367, the latter 416 pages. The alterations are made in two ways :—*first*, by small additions and occasional omissions here and there; *secondly*, by the insertion of a few articles of considerable length. Of the first kind we may notice such changes as these :—the insertion of the proof of a condition that $Pdx + Qdy$ may be an exact differential (p. 143); the recasting and extension of the article on the curve $r^m = a^m \cos m\theta$ (p. 227); the additional examples of algebraical maxima and minima (p. 163); the additions to and omissions from the examples on Partial Differential Coefficients (pp. 137–40), and so on. Of the second kind may be mentioned :—two long extracts from M. Navier's *Leçons d'Analyse* on the principles of the Differential Calculus (pp. 7–10), and on the Failure of Taylor's Theorem

(pp. 405, 406); a large addition to the articles on Infinitesimals (pp. 40–43); and a new article on the direction of the normal in Vectorial Coordinates (pp. 230–233). The additions of most importance, however, are two—an appendix containing a brief geometrical discussion of the chief properties of the Cartesian Oval (pp. 411–416), and the account of Rolling Curves or Roulettes. In the second edition the subject fills about ten pages at the end of the chapter on tracing Curves; in the present edition these ten pages are expanded into a separate chapter (XIX.) of twenty nine pages, which gives a fairly complete view of the questions suggested by cycloids and analogous curves, such as might be looked for in a work which professedly treats of the Theory of Plane Curves, together with some kinematical applications. On the whole it is plain that these additions are not such as to change very materially the general character of the work; they serve, however, to render it somewhat more complete, and to make it still more worthy of the attention of every student of geometry.

XXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 153.]

May 4, 1876.—Capt. F. J. O. Evans, R.N., C.B., Vice-President, in the Chair.

THE following communication was read:—
  " On the Origin of Windings of Rivers in Alluvial Plains, with Remarks on the Flow of Water round Bends in Pipes." By Professor James Thomson, LL.D., F.R.S.E.

In respect to the origin of the windings of rivers flowing through alluvial plains, people have usually taken the rough notion that when there is a bend in any way commenced, the water just rushes out against the outer bank of the river at the bend, and so washes that bank away, and allows deposition to occur on the inner bank, and thus makes the sinuosity increase. But in this they overlook
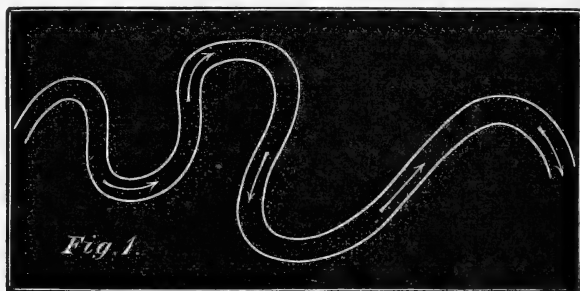
Fig. 1.

the hydraulic principle, not generally known, that a stream flowing along a straight channel and thence into a curve must flow with a diminished velocity along the outer bank, and an increased velocity along the inner bank, if we regard the flow as that of a perfect fluid. In view of this principle, the question arose to me some years ago:—*Why does not the inner bank wear away more than*

the outer one ?   We know by general experience and observation that in fact the outer one does wear away, and that deposits are often made along the inner one.   *How does this arise* ?
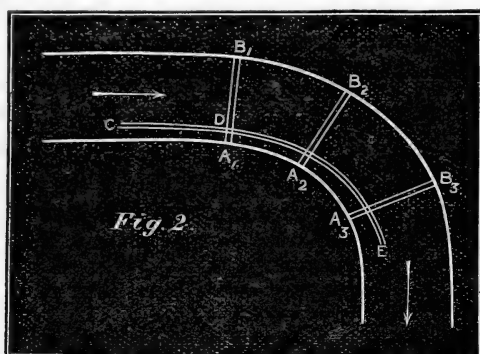
Fig. 2.

The explanation occurred to me in the year 1872, mainly as follows:—For any lines of particles taken across the stream at different places, as A_1B_1, A_2B_2, &c. in fig. 2, and which may be designated in general as AB, if the line be level, the water-pressure must be increasing from A to B, on account of the centrifugal force of the particles composing that line or bar of water; or, what comes to the same thing, the water-surface of the river will have a transverse inclination rising from A to B.   The water in any stream-line C D E* at or near the surface, or in any case not close to the bottom, and flowing nearly along the inner bank, will not accelerate itself in entering on the bend, except in consequence of its having a *fall of free level* in passing along that stream-line †.

But the layer of water along the bottom, being by friction much retarded, has much less centrifugal force in any bar of its particles extending across the river; and consequently it will flow sidewise along the bottom towards the inner bank, and will, part of it at least, rise up between the stream-line and the inner bank, and will protect the bank from the rapid scour of that stream-line and of other adjacent parts of the rapidly flowing current; and as the sand and mud in motion at the bottom are carried in that bottom layer, they will be in some degree brought in to that inner bank, and may have a tendency to be deposited there.

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* This, although here conveniently spoken of as a stream-line, is not to be supposed as having really a steady flow.   It may be conceived of as an average stream-line in a place where the flow is disturbed with eddies or by the surrounding water commingling with it.

† It must be here explained that by the *free level* for any particle is to be understood the level of the atmospheric end of a column, or of any bar, straight or curved, of particles of statical water, having one end situated at the level of the particle, and having at that end the same pressure as the particle has, and having the other end consisting of a level surface of water freely exposed to the atmosphere, or else having otherwise atmospheric pressure there; or, briefly, we may say that the *free level* for any particle of water is the level of the atmospheric end of its *pressure-column*, or of an equivalent ideal pressure-column.

On the other hand, along the outer bank there will be a general
tendency to descent of surface-water which will have a high velo-
city, not having been much impeded by friction ; and this will wear
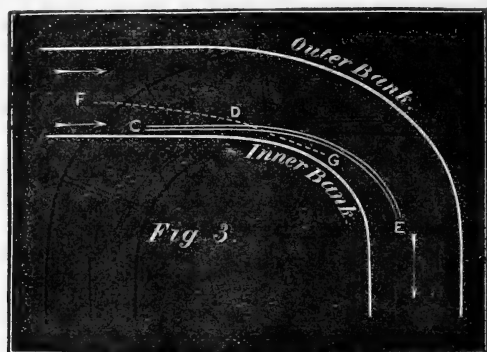
away the bank and carry the worn substance in a great degree down
to the bottom, where, as explained before, there will be a general
prevailing tendency towards the inner bank.

Now, further, it seems that even from the very beginning of the
curve forward there will thus be a considerable protection to the
inner bank.   Because a surface stream-line C D, or one not close to
the bottom, flowing along the bank which in the bend becomes the
inner bank, will tend to depart from the inner bank at D, the com-
mencement of the bend, and to go forward along D E, or by some such
course, leaving the space G between it and the bank to be supplied
by slower-moving water which has been moving along the bottom of
the river perhaps by some such oblique path as the dotted line F G.

It is further to be observed that ordinarily or very frequently
there will be detritus travelling down stream along the bottom and
seeking for resting-places, because the cases here specially under
consideration are only such as occur in alluvial plains ; and in
regions of that kind there is ordinarily *, on the average, more de-
position than erosion.   This consideration explains that we need
not have to seek for the material for deposition on the inner bank
in the material worn away from the outer bank of the same bend
of the river.   The material worn from the outer bank may have to
travel a long distance down stream before finding an inner bank
of a bend on which to deposit itself.   And now it seems very clear
that in the gravel, sand, and mud carried down stream along the
bottom of the river to the place where the bend commences, there
is an ample supply of detritus for deposition on the inner bank of
the river even at the earliest points in the curve which will offer any
resting-place.   It is especially worthy of notice that the oblique
flow along the bottom towards the inner bank begins even up stream
from the bend, as already explained, and as shown by the dotted
line F G in fig. 3.   The transverse movement comprised in this

* That is to say, except when by geological changes the causes which have
been producing the alluvial plain have become extinct, and erosion by the river
has come to predominate over deposition.

oblique flow is instigated by the abatement of pressure, or lowering of free level, in the water along the inner bank produced by centrifugal force in the way already explained.

It may now be remarked that the considerations which have in the present paper been adduced in respect to the mode of flow of water round a bend of a river, by bringing under notice, conjointly, the lowering of free level of the water at and near the inner bank, and the raising of free level of the water at and near the outer bank relatively to the free level of the water at middle of the stream, and the effect of retardation of velocity in the layer flowing along the bed of the channel in diminishing the centrifugal force in the layer retarded, and so causing that retarded water, and also frictionally retarded water, even in a straight channel of approach to the bend, to flow obliquely towards the inner bank, tend very materially to elucidate the subject of the mode of flow of water round bends in pipes, and the manner in which bends cause augmentation of frictional resistance in pipes, a subject in regard to which I believe no good exposition has hitherto been published in any printed books or papers; but about which various views, mostly crude and misleading, have been published from time to time, and are now often repeated, but which, almost entirely, ought to be at once rejected.

June 15.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

The following communication was read:—

" On Repulsion resulting from Radiation.   Influence of the Residual Gas."—(Preliminary Notice.) By William Crookes, F.R.S. &c.

I have recently been engaged in experiments which are likely to throw much light on some obscure points in the theory of the repulsion resulting from radiation. In these I have been materially assisted by Professor Stokes, both in original suggestions and in the mathematical formulæ necessary for the reduction of the results.   Being prevented by other work from completing the experiments sufficiently to bring them before the Royal Society prior to the close of the session, I have thought that it might be of interest were I to publish a short abstract of the principal results I have obtained, reserving the details until they are ready to be brought forward in a more complete form.

In the early days of this research, when it was found that no movement took place until the vacuum was so good as to be almost beyond the powers of an ordinary air-pump to produce, and that as the vacuum got more and more nearly absolute, so the force increased in power, it was justifiable to assume that the action would still take place when the minute trace of residual gas which theoretical reasoning proved to be present was removed. The first and most obvious explanation therefore was that the repulsive force was directly due to radiation.   Further consideration, however, showed that the very best vacuum which I had succeeded in producing might contain enough matter to offer considerable resistance to motion.   I have already pointed out that in some experiments, where the rarefaction was pushed to a very high point, the torsion-beam appeared to be swinging in a

viscous fluid (194) ; and this at once led me to think that the re-
pulsion caused by radiation was indirectly due to a difference of
thermometric heat between the black and white surfaces of the
moving body (195), and that it might be due to a secondary action
on the residual gas.

On April 5, 1876, I exhibited at the Soirée of the Royal Society
an instrument which proved the presence of residual gas in a
radiometer which had been exhausted to a very high point of sen-
sitiveness.   A small piece of pith was suspended to one end of a
cocoon fibre, the other end being attached to a fragment of steel.
An external magnet held the steel to the inner side of the glass
globe, the pith then hanging down like a pendulum, about a mil-
limetre from the rotating vanes of the radiometer.   By placing
a candle at different distances off, any desired velocity, up to several
hundreds per minute, could be imparted to the fly of the radiometer.
Scarcely any movement of the pendulum was produced when the
rotation was very rapid ; but on removing the candle, and letting
the rotation die out, at one particular velocity the pendulum set
up a considerable movement.   Professor Stokes suggested (and,
in fact, tried the experiment at the time) that the distance of the
candle should be so adjusted that the permanent rate of rotation
should be the critical one for synchronism corresponding to the
rate at which one arm of the fly passed for each complete oscil-
lation.   In this way the pendulum was kept for some time swinging
with regularity through a large arc.

This instrument proved that, at a rarefaction so high that the
residual gas was a non-conductor of an induction-current, there was
enough matter present to produce motion, and therefore to offer
resistance to motion.   That this residual gas was something more
than an accidental accompaniment of the phenomena was rendered
probable by the observations of Dr. Schuster, as well as by my own
experiments on the movement of the floating glass case of a radio-
meter when the arms are fixed by a magnet*.

My first endeavour was to get some experimental means of dis-
criminating between the viscosity of the minute quantity of re-
sidual gas and the other retarding forces, such as the friction of
the needle-point on the glass cup when working with a radiometer,
or the torsion of the glass fibre when a torsion-apparatus was used.
A glass bulb is blown on the end of a glass tube, to the upper
part of which a glass stopper is accurately fitted by grinding.   To
the lower part of the stopper a fine glass fibre is cemented, and to
the end of this is attached a thin oblong plate of pith, which hangs
suspended in the centre of the globe ; a mirror is attached to the
pith bar, which enables its movement to be observed on a
graduated scale.   The stopper is well lubricated with the burnt
india-rubber which I have already found so useful in similar
cases (207).   The instrument is held upright by clamps, and is
connected to the pump by a long spiral tube.   The stopper is fixed
rigidly in respect to space, and an arrangement is made by which
the bulb can be rotated through a small angle.   The pith plate,

* Proc. Roy. Soc. vol. xxiv. p. 409.

with mirror, being suspended from the stopper, the rotation of the bulb can only cause a motion of the pith through the intervention of the enclosed air. Were there no viscosity of the air, the pith would not move; but if there be viscosity, the pith will turn in the same direction as the bulb, though not to the same extent, and, after stopping the vessel, will oscillate backwards and forwards in decreasing arcs, presently setting in its old position relatively to space.

It was suggested by Prof. Stokes that it would be desirable to register not merely the amplitude of the first swing, but the readings of the first five swings or so. This would afford a good value of the logarithmic decrement (the decrement per swing of the logarithm of the amplitude of the arcs), which is the constant most desirable to know. The logarithmic decrement will involve the viscosity of the glass fibre; but glass is so nearly perfectly elastic, and the fibre so very thin, that this will be practically insensible.

According to Professor Clerk Maxwell, the viscosity of a gas should be independent of its density; and the experiments with this apparatus have shown that this is practically correct, as the logarithmic decrement of the arc of the oscillation (a constant which may be taken as defining the viscosity of the gas) only slightly diminishes up to as high an exhaustion as I can conveniently attain—higher, indeed, than is necessary to produce repulsion by radiation.

I next endeavoured to measure, simultaneously with the logarithmic decrement of the arc of oscillation, the repulsive force produced by a candle at high degrees of exhaustion. The motion produced by the rotation of the bulb alone has the advantage of exhibiting palpably to the eye that there *is* a viscosity between the suspended body and the vessel; but once having ascertained that, and admitting that the logarithmic decrement of the arc of oscillation (when no candle is shining on the plate) is a measure of the viscosity, there is no further necessity to complicate the apparatus by having the ground and lubricated stopper. A movement of the whole vessel bodily through a small arc is equally effective for getting this logarithmic decrement; and the absence of the stopper enables me to have the whole apparatus sealed up in glass, and I can therefore experiment at higher rarefactions than would be possible when a lubricated stopper is present.

The apparatus, which is too complicated to describe without a drawing, has attached to it:—*a*, a Sprengel pump; *b*, an arrangement for producing a chemical vacuum; *c*, a lamp with scale, on which to observe the luminous index reflected from the mirror; *d*, a standard candle at a fixed distance; and *e*, a small vacuumtube, with the internal ends of the platinum wires close together. I can therefore take observations of:—

1. The logarithmic decrement of the arc of oscillation when under no influence of radiation;

2. The successive swings and final deflection when a candle shines on one end of the blackened bar;

3. The appearance of the induction-spark between the platinum wires.

1 measures the viscosity; 2 enables me to calculate the force of radiation of the candle; and 3 enables me to form an idea of the progress of the vacuum according as the interior of the tube becomes uniformly luminous, striated, luminous at the poles only, or black and non-conducting.

The apparatus is also arranged so that I can try similar experiments with any vapour or gas.

The following are some of the most important results which this apparatus has as yet yielded.

Up to an exhaustion at which the gauge and barometer are sensibly level, there is not much variation in the viscosity of the internal gas (dry atmospheric air). Upon now continuing to exhaust, the force of radiation commences to be apparent, the viscosity remaining about the same. The viscosity next commences to diminish, the force of radiation increasing. After long-continued exhaustion the force of radiation approaches a maximum; but the viscosity measured by the logarithmic decrement begins to fall off, the decrease being rather sudden after it has once commenced.

Lastly, some time after the logarithmic decrement has commenced to fall off, and when it is about one fourth of what it was at the commencement, the force of radiation diminishes. At the highest exhaustion I have yet been able to work at, the logarithmic decrement is about one twentieth of its original amount, and the force of repulsion has sunk to a little less than one half of the maximum. The attenuation has now become so excessive that we are no longer at liberty to treat the number of gaseous molecules present in the apparatus as practically infinite; and, according to Professor Clerk Maxwell's theory, the mean length of path of the molecules between their collisions is no longer very small compared with the dimensions of the apparatus.

The degree of exhaustion at which an induction-current will not pass is far below the extreme exhaustions at which the logarithmic decrement falls rapidly.

The force of radiation does not act suddenly, but takes an appreciable time to attain its maximum—thus proving, as Prof. Stokes has pointed out, that the force is not due to radiation *directly* but *indirectly*.

In a radiometer exhausted to a very high degree of sensitiveness, the viscosity of the residual gas is almost as great as if it were at the atmospheric pressure.

With other gases than air the phenomena are different in degree, although similar in kind—aqueous vapour, for instance, retarding the force of repulsion to a great extent, and carbonic acid acting in a similar though less degree.

The evidence afforded by the experiments of which this is a brief abstract is to my mind so strong as almost to amount to conviction that the repulsion resulting from radiation is due to an action of thermometric heat between the surface of the moving

body and the case of the instrument, through the intervention of the residual gas. This explanation of its action is in accordance with recent speculations as to the ultimate constitution of matter and the dynamical theory of gases.

GEOLOGICAL SOCIETY.
[Continued from p. 156.]
January 24th, 1877.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

The following papers were read :—
1. " Note on the Question of the Glacial or Volcanic Origin of the Talchír Boulder-bed of India and the Karoo Boulder-bed of South Africa." By H. F. Blanford, Esq., F.G.S.

The author, referring to a doubt expressed by the President in a paper on Australian Tertiary Corals as to the glacial origin of the Talchír Boulder-bed, indicated that the hypothesis of its formation by the action of local glaciers under present climatal conditions would require the elevation of the whole region to the extent of 14,000 or 15,000 feet, and the assumption that the denudation of this great mountain mass was so moderate that large tracts of the ancient surface are still preserved at levels now only a few hundred feet above the sea. This the author regarded as very improbable. He assumed that the President, rejecting the evidence adduced by various writers in favour of the glacial origin of the Talchír and Karoo Boulder-beds, was inclined to fall back upon the notion of their being of volcanic origin, and quoted a letter from Mr. King, who had described the Talchír rocks of Kámáram as trappean, in which that gentleman stated that the rocks so interpreted by him prove to be dark green and brownish mudstone. He cited further evidence of like nature, and concluded that the ascription of a volcanic origin to these boulder-beds was probably in all cases due to similar misinterpretations.

2. " On British Cretaceous Patelloid Gasteropoda." By John Starkie Gardner, Esq., F.G.S.

3. " Observations on remains of the Mammoth and other Mammals from Northern Spain." By A. Leith Adams, Esq., M.B., F.R.S., F.G.S.

February 7th, 1877.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

The following paper was read :—
1. " On the Chemical and Mineralogical Changes which have taken place in certain Eruptive Rocks of North Wales." By John Arthur Phillips, Esq., F.G.S.

In this paper the author described the felspathic rock of Penmaenmawr, which has been erupted through Silurian strata, and rises to a height of 1553 feet above the level of the sea. The rock, which is composed of crystalline felspar with minute crystals of some hornblendic mineral, is fine-grained and greenish grey, divided into beds by joints dipping north at an angle of about 45°, and again divided by double jointings, sometimes so developed as to

render the rock distinctly columnar. At the eastern end of the mountain the stone is so close in texture as often almost to resemble chert. In the next two quarries westward the rock is coarser, and its jointing less regular. In the most westerly quarry the stone is generally fresher in appearance, closer in grain, and greener in colour. All these stones are probably modifications of the same original rock. From the chemical analysis of the rocks the author concludes that, supposing them all to have had originally the same composition as the unaltered rock in the most westerly quarry, that at the extreme east of the mountain has lost about 3 per cent. of silica, and the others have received respectively an increase of 1·35 and 0·77 per cent. of silica. The altered rocks contain an abundance of quartz granules, due probably to the crystallization of progressively dissociated silica, as the specimens of rock in which these granules occur do not contain a larger proportion of silica than those in which its presence can hardly be detected under the microscope. The proportion of alkalies in the different specimens does not materially vary.

Overlying the second quarry at the east end of the mountain is an ash bed of reddish brown colour, containing more than 10 per cent. of protoxide of manganese and nearly 20 per cent. of peroxide of iron, and showing a great diminution in the percentage of silica when compared with the associated crystalline rock.

The author further described the characters of the uralite-porphyry of the Mawddach valley near Dolgelly, which is of a greyish-green colour, spotted with black, and consists of a granular base enclosing patches and crystals of uralite, the outlines of which are sometimes sharp and well defined, but generally rounded and merging into the general base.

XXXII. *Intelligence and Miscellaneous Articles.*

ON THE DETERMINATION OF THE POLAR DISTANCE IN MAGNETS.
BY R. BENOIT.

POUILLET had the notion of making use of the equation of equilibrium of a magnetized bar placed in given conditions (into which equation its polar distance enters), in order to determine the precise place occupied by each of the poles[*]. The following method is both more direct and more general than that which he employed.

Let AB, A'B' be two magnetized bars placed horizontally one above the other so that their centres O, O' are in the same vertical line. The first is fixed; the second can turn freely about the axis O O'. As long as the distance separating them is sufficient, the resultants of the reciprocal actions of their magnetic elements pass very evidently through the poles A, B, A', B'. Now these actions give rise (1) to symmetrical vertical forces destroyed by the weight and the resistance of the suspending wire, and (2) to a horizontal couple which tends to cause the movable magnet to rotate, and of which the expression is easily found.

Let m, m' denote the quantities of free magnetism which we may

[*] *Comptes Rendus de l'Académie des Sciences*, Feb. 5, 1865.

suppose condensed at the respective poles of the two magnets, $2l$ the polar distance of the fixed magnet, $2l'$ the polar distance of the movable magnet, d the distance (O, O') of the centres of the two magnets, and δ the angle made by A'B' with the vertical plane containing AB; the moment of the horizontal couple is represented by the formula

$$M = 2mm'll' \sin \delta [(d^2 + l^2 + l'^2 + 2ll' \cos \delta)^{-\frac{3}{2}} + (d^2 + l^2 + l'^2 - 2ll' \cos \delta)^{-\frac{3}{2}}].$$

The movable bar, submitted simultaneously to the influence of the earth and to that of the fixed bar, generally takes a new position of equilibrium, and makes an angle ϕ with the magnetic meridian. The moment of the earth-couple, $2m'l'H \sin \phi$, is then equal and contrary to M.

This being admitted, let us suppose AB rotated about the axis OO' until its direction is perpendicular to that of A'B'; δ will then be equal to $\frac{\pi}{2}$; and consequently we shall have, for any position of equilibrium,
$$H \sin \phi = 2ml(d^2 + l^2 + l'^2)^{-\frac{3}{2}},$$
whence
$$\sin \phi (d^2 + l^2 + l'^2)^{-\frac{3}{2}} = \frac{2ml}{H}.$$

The conditions of this experiment are easily realized by an arrangement similar to that of the sine-compass: we need only to replace the movable multiplier of the latter instrument by a support to hold the fixed magnet, and upon which it can be fixed at different heights.

If the magnet AB be transferred to another distance d', ϕ becomes ϕ', and we have
$$\sin \phi' (d'^2 + l^2 + l'^2)^{\frac{3}{2}} = \frac{2ml}{H}.$$

These two formulæ give

$$l^2 + l'^2 = \frac{d^2 \sin^{\frac{2}{3}} \phi - d'^2 \sin^{\frac{2}{3}} \phi'}{\sin^{\frac{2}{3}} \phi' - \sin^{\frac{2}{3}} \phi}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

To obtain a second equation between l^2 and l'^2, the interposition of a third bar having $2l''$ for its polar distance will be sufficient. Operating upon bars 1 and 2 we shall have
$$l^2 + l'^2 = A;$$
in like manner bars 1 and 3 will give
$$l^2 + l''^2 = B;$$
and lastly, bars 2 and 3, $\quad l'^2 + l''^2 = C.$

From these three equations l, l', l'' will be obtained.

The following experiments, carried out merely for the purpose of verification, and with apparatus which permitted only moderate precision in the measurements, nevertheless conducted to very concordant results when the means of the observations were taken. They were made upon four needles of tempered steel, magnetized to saturation, 1·3 millim. in diameter, and the lengths of which were 18, 16, 14, and 12 centims.

For want of space I cannot give here the details of the observations, but only the result of the calculations—that is to say, the values of l, l', l'' for the three needles, combining the series of observations successively in threes :—

		Series		
	A, B, D. millim.	A, C, E. millim.	B, C, F. millim.	D, E, F. millim.
Bar I.	78·1	78·3	78·1	
„ II.	70·0	69·8	....	69·8
„ III.	61·6	....	61·7	61·9
„ IV.	....	49·6	49·9	49·6

I may remark that, once the polar distances are determined, the same experiments will furnish the value of the ratio $\dfrac{2ml}{H}$, necessary in order to measure the intensity of terrestrial magnetism by Gauss's method.—*Comptes Rendus de l'Académie des Sciences,* Jan. 8, 1877, tome lxxxix, pp. 76–78.

PHOTOGRAPHS OF THE SPECTRA OF VENUS AND α LYRÆ.
NOTE BY PROF. HENRY DRAPER, M.D.

Since the spring of 1872 I have been making photographs of the spectra of the stars, planets, and Moon, and particularly, among the stars, of α Lyræ and α Aquilæ, with my 28-inch reflector and 12-inch refractor. In the photograph of α Lyræ, bands or broad lines are visible in the violet and ultra-violet region unlike any thing in the solar spectrum. The research is difficult and consumes time, because long exposures are necessary to impress the sensitive plate, and the atmosphere is rarely in the best condition. The image of a star or planet must be kept motionless for from ten to twenty minutes ; and hence the driving-clock of the telescope is severely taxed.

During last summer I obtained good results, and in October took photographs of the spectrum of Venus which show a large number of lines. I am now studying these pictures, and have submitted them to the inspection of several of my scientific friends, among others Professors Barker, Langley, Morton, and Silliman. There seems to be in the case of Venus a weakening of the spectrum toward H and above that line, of the same character as that I have photographically observed to take place in the spectrum of the Sun near sunset.—Silliman's *American Journal,* February 1877.

ON THE SPECTRA OF METALS AT THE BASE OF FLAMES.
BY M. GOUY.

It is known that a flame produced by a mixture of coal-gas and air, in proportions suitable for burning without the help of the external air, has for its base an inner cone, at the surface of which the combustion commences. This surface is brilliant, of a blue or green colour, and gives the spectrum of carbon. The experiments which I am about to relate show that this same surface gives a very different spectrum from that of the flame of which it forms the base when the combustible mixture holds in suspension saline powders.

The saline solutions are pulverized by a jet of compressed air; the air, charged with powder, enters a regulator, into which the gas also comes, and whence a mixture issues of constant composition. This mixture passes into a vertical tube of 19 millims. diameter, capped with iron-wire gauze, above which it burns with a conical flame 6–8 centims. in height. The height of the inner cone varies from 3 or 4 centims. to zero; and the flame can be rendered oxidizing or reducing.

By means of a lens the image of the flame is thrown upon the slit of the spectroscope. Two spectra are then seen, the one above the other. The lower spectrum is produced by the light of the blue surface; and all the lines of which it is composed remain at exactly the same height. The other is produced by the flame proper; and the lines belonging to it encroach upon the lower spectrum, by reason of the shape of the flame.

When the apparatus is employed void of powder, the lower spectrum shows brightly the carbon-lines. If we pulverize a solution of chloride of lithium, the following is observed:—The upper spectrum shows a very bright red line and a feeble line in the orange. The first of these appears of equal brightness throughout; the other becomes much brighter just at the point where it penetrates into the lower spectrum. Moreover the lower spectrum shows distinctly a blue line (γ of the electrical spectrum), which terminates at the same height as the carbon-lines and is absent from the upper spectrum.

These characters will be again found with other metals:—

Sulphate of Thallium.—This gives its characteristic green line, which is clearly strengthened on penetrating into the lower spectrum.

Chloride of Calcium.—The upper spectrum is deprived of the line proper to the undecomposed chloride. The blue line is a little strengthened on entering the second spectrum.

Chloride of Strontium.—The upper spectrum offers nothing peculiar; the lower shows three faint blue lines, which belong to the electric spectrum of strontium.

Chloride of Barium.—The lines and bands of the upper spectrum are reinforced on penetrating into the other, especially the bright green line.

Chloride of Magnesium.—The upper spectrum shows traces of the lines α and γ of the electric spectrum; the latter is more visible when the flame is oxidizing. The spectrum gives the line α very bright, and close by the green line of carbon. This line is triple in the electric spectrum; I have only been able to distinguish two of the components, the third being doubtless confused with the carbon line. Of these two components the brightest is as bright as the latter; and as it is very near it, one can verify that they have the same height. The line γ is not reinforced in the lower spectrum.

Chloride of Iron.—The flame is pretty luminous, greenish yellow. The upper spectrum consists of bands and numerous fine lines upon a continuous ground; the lower one shows three groups of sufficiently visible lines, corresponding to the groups β and ζ of the electric spectrum between the green and the blue, and γ in the violet.

Chloride of Cobalt.—The flame very much resembles the preceding. The upper spectrum is continuous, and fairly bright; the lower shows three faint lines in the violet and the extreme violet (γ, θ, and η of the electric spectrum).

Chloride of Zinc.—Nothing more than a feeble continuous luminosity in the upper spectrum; the lower shows pretty clearly the violet line α of the electric spectrum.

Chloride of Cadmium.—Nothing in the upper spectrum; the other presents a feeble violet line, β of the electric spectrum.

Nitrate of Manganese.—There is nothing particular in the upper spectrum; the lower shows a group of three feeble violet lines, α of the electric spectrum.

Nitrate of Copper.—The upper spectrum shows several bands; one of them, in the red, is notably reinforced in the lower spectrum.

Nitrate of Lead.—Nothing in the upper spectrum; the other presents a very distinct line in the extreme violet, α of the electric spectrum.

Nitrate of Silver.—Nothing in the upper spectrum; the other shows two well-marked lines, α and β of the electric spectrum.

Chloride of Platinum.—The flame is of a bluish whiteness; its illumination is equal to that of a wax taper. The upper spectrum shows a continuous bright luminosity, with some feeble bands and lines. The lower does not resemble the electric spectrum; it is formed of a fine series of sufficiently brilliant bands, less bright on the side towards the red, their other edge being clean; a few fainter lines are also seen.

Of the other metals, some have not been submitted to experiment, and others have not given any clear results.

The spectra above described were observed with flames in some degree reducing. On charging the flame with gas the interior cone lengthens, and its spectrum becomes less bright without changing its nature. With a large excess of air the cone changes its form, dividing into violet points, sometimes very tall. At this moment the lines of carbon have disappeared, save that in the violet; they are replaced by a continuous ground, on which the metallic lines, enfeebled, detach themselves. The flame properly so called is almost invisible, and shows scarcely a trace of the sodium-line; but it becomes visible and coloured green when it contains copper.

In brief, we have seen that the base of the flame gives, over a very small elevation, a spectrum which approaches the electric spectrum of the same metal; I purpose to extend these researches to other flames.

I will remark in conclusion, that in the usual spectral analyses a mixture of both spectra (of the cone and the flame) is seen; therefore the relative intensity of the lines must change according to the part of the flame which is viewed, as Lecoq de Boisbaudran has observed for the chloride of manganese *.

This investigation was made in the laboratory of M. Desains, at the Sorbonne.—*Comptes Rendus de l'Académie des Sciences*, Jan. 29, 1877, tome lxxxiv. pp. 231–234.

* *Spectres Lumineux*, p. 122. From this work we have borrowed the letters assigned to the lines.

XXXIII. *On the Theory of an Imperfectly Homogeneous Elastic Solid. By* Professor C. NIVEN, *Queen's College, Cork*[*].

1. THE present paper attempts to deal with the theory of a solid composed of elastic material, homogeneous on a large scale but not homogeneous when considered in detail. It may be likened, to use Sir W. Thomson's comparison, to a wall built of rubble, which appears homogeneous enough when looked at from a sufficiently great distance. Inside such a substance the stress across a small plane area will not be a simple force, but may, under certain circumstances, consist of a force and a couple. In fact the investigation was suggested by the theories of the bending and twisting of wires and of the bending of thin plates, in which the stress is thus specified.

On forming the equations which determine the small motions of the solid, I found that it was possible to frame a mathematical illustration of the phenomenon of the circular polarization of light in quartz and other substances—and that if we admit that the substance transmits in every direction two waves consisting of transversal vibrations, these waves must necessarily be elliptic and oppositely polarized, according to Professor Stokes's definition. In certain directions the ellipses become circles. The results are thus far the same as those obtained by Clebsch in his geometrical development of Cauchy's theory; but the method of solving the equations of motion is quite different, and the physical foundations of the two theories have nothing in common. It is unnecessary, there-

fore, to dwell upon Cauchy's or more recent theories, which, though more complete, possess some of its general features.

One result of the present investigations, which is a little startling, is, that part of the energy of the substance resides at its surface. It may be that a more complete investigation might show that this part of the energy must necessarily vanish; but I have preferred to leave for the present untouched the question as to whether any or what physical interpretation might be given of these terms.

The coefficients which produce circular polarization contain as a factor a quantity of the order of the molecular distance; and, conversely, the phenomena of the rotation of the plane of polarization in quartz may be applied to form a rough estimate of this distance. The result is not very widely different from that which Sir W. Thomson and others have deduced from other considerations.

2. In dealing with the bending of a wire the mathematical element chosen is a length ds of the wire bounded by two cross sections; and the stress which acts on the element across one of these sections consists of a force and a couple, and not, as in the case of the element-area in the interior of a homogeneous elastic solid, of a force only. In the case of a lamina, the stress acting across any small element of a normal section consists also of a force and a couple. These couples, which, as already observed, do not present themselves in specifying the stress inside a homogeneous elastic solid, arise from the unequal contractions and expansions of the substance at different points of the normal section of the plate, or cross section of the wire. Moreover the mathematical machinery used is of a coarser type than that employed in dealing with a homogeneous solid—coarser in one dimension in the case of the plate, and in two dimensions for the wire. Now if the constitution of a solid itself be such that, though sensibly homogeneous on a large scale, it is really heterogeneous when examined more closely, the forces across two neighbouring indefinitely small parallel areas may not be parallel and proportional to the areas. If, therefore, the mathematical element were chosen large enough to eliminate all trace of this coarseness of structure, the actions on the faces of the element might not result each in a definite force but in a force and a couple— what Dr. Ball calls a " wrench," or even in stress of a more complex type. Sir W. Thomson has pointed out in ' Nature ' (vol. i. p. 551) that this discontinuity is what is established in Cauchy's theory of the dispersion of light; and he has even used Cauchy's results to obtain a numerical estimate of the intermolecular spaces of matter.

Mathematical theories of an apparently continuous substance are sometimes based upon finer molecular theories. The distribution, however, of the molecules and of their properties presents so many irregularities and discontinuities, that it is impossible to trace the changes which take place in each individual, and we are thus obliged to confine ourselves to taking the average of a large number of them ; and it is this average with which we are concerned in explaining the sensible properties of matter. In this way the dynamical theory of gases has been constructed; and the earlier theories of elasticity rested on a similar foundation. Green's theory of elasticity, on the other hand, is based on the general consideration that, whatever be the nature of the forces acting in the interior of a solid, the energy required to produce the change of form of an element-volume depends only on the deformations which it experiences. These may be expressed in the following manner. If u, v, w be the displacements of a point (x, y, z), $u+\Delta u$, $v+\Delta v$, $w+\Delta w$ those of an adjacent point $(x+h, y+k, z+l)$, then

$$\Delta u = h\frac{du}{dx} + k\frac{du}{dy} + l\frac{du}{dz}, \text{ &c.,}$$

provided we neglect squares and products of h, k, l. These terms may be analyzed (after Helmholtz) as follows:—Putting

$$\left.\begin{array}{l}
\mathrm{A}=\dfrac{du}{dx}, \quad \mathrm{B}=\dfrac{dv}{dy}, \ . \ . \ 2\mathrm{D}=\dfrac{dw}{dy}+\dfrac{dv}{dz}, \ . \ . \ 2\mathrm{F}=\dfrac{dv}{dx}+\dfrac{du}{dy}, \\[2mm]
2\varpi_1=\dfrac{dw}{dy}-\dfrac{dv}{dz}, \quad 2\varpi_2=\dfrac{du}{dz}-\dfrac{dw}{dx}, \quad 2\varpi_3=\dfrac{dv}{dx}-\dfrac{du}{dy},
\end{array}\right\} \ . \ (1)$$

we shall have

$$\left.\begin{array}{l}
\Delta u = \mathrm{A}h + \mathrm{F}k + \mathrm{E}l - \varpi_3 k + \varpi_2 l, \\[1mm]
\Delta v = \mathrm{F}h + \mathrm{B}k + \mathrm{D}l - \varpi_1 l + \varpi_3 h, \\[1mm]
\Delta w = \mathrm{E}h + \mathrm{D}k + \mathrm{C}l - \varpi_2 h + \varpi_1 k.
\end{array}\right\} \ \cdot \ \cdot \ \cdot \ (2)$$

The terms depending on ϖ_1, ϖ_2, ϖ_3 represent a rotation of the parts of the medium near (x, y, z) round lines parallel to the axes ; and when the motions of the solid are exceedingly small compared with the length of a wave in it, the remaining terms express a pure strain of the substance in the neighbourhood of (x, y, z). The energy contained in the element is a function of A, B, C, D, E, F.

This theory, which compares the mathematical element with a homogeneously strained solid, assumes that the element may be taken so small that the enclosed solid may be treated as homogeneously strained, and that the whole distortion of the

R 2

solid may be represented by supposing the elements of this
strain and rotation to vary continuously from point to point.
If, however, the solid be not perfectly homogeneous in detail,
though it may be so on a large scale, it will be necessary to
take the element-volume great enough to eliminate the effect
of the irregular variation of the different particles of the solid
within the element itself. In order to understand how this
may be represented, let us first suppose that A, B, C, D, E, F,
ϖ_1, ϖ_2, ϖ_3 vary regularly and continuously within the element;
then, if these symbols refer to some definite point $P(x, y, z)$ in
its interior, say its centre of gravity, the value of ϖ_1 at some
other point P_1 $(x+h, y+k, z+l)$ will be denoted by

$$\varpi_1 + h\frac{d\varpi_1}{dx} + k\frac{d\varpi_1}{dy} + l\frac{d\varpi_1}{dz}.$$

But even if we admit that these quantities may vary gradually,
though not necessarily regularly, we may suppose that the
value of ϖ_1 at P_1 differs from its value at P by linear func-
tions of the differential coefficients of itself and of the remain-
ing elements of the distortion, and that the coefficients are
quantities of the order of the magnitude ($\mathfrak{m}$) which measures
the extent through which the coarseness of structure is sensible
—that is to say, the molecular distance. Thus the circum-
stances of the strain of the element-volume will require for
their complete specification the nine quantities above men-
tioned, and also their twenty-seven differential coefficients with
regard to x, y, z.

3. Now the known cogrediency of ϖ_1, ϖ_2, ϖ_3, with the dis-
placements u, v, w, enables us to analyze their differential
coefficients into the two groups:—

$$\left. \begin{array}{l} 2\dfrac{d\varpi_1}{dx}, \ 2\dfrac{d\varpi_2}{dy}, \ 2\dfrac{d\varpi_3}{dz}, \dfrac{d\varpi_3}{dy} + \dfrac{d\varpi_2}{dz}, \dfrac{d\varpi_1}{dz} + \dfrac{d\varpi_3}{dx}, \dfrac{d\varpi_2}{dx} + \dfrac{d\varpi_1}{dy}; \\[2mm] \dfrac{d\varpi_3}{dy} - \dfrac{d\varpi_2}{dy}, \dfrac{d\varpi_1}{dz} - \dfrac{d\varpi_3}{dx}, \dfrac{d\varpi_2}{dx} - \dfrac{d\varpi_1}{dy}. \end{array} \right\} (3)$$

The first group are cogredient with strains, and will be denoted
by a, b, c, d, e, f; they represent three uniform twists of the
element about three rectangular axes. The second group are
cogredient with displacements, and will be denoted by $2\varpi'_1$,
$2\varpi'_2$, $2\varpi'_3$. With respect to the differential coefficients of A,
B, ... F, I have not attempted to analyze them further; but
this is of no consequence, as they will give rise to terms in
our equations which will be subjected in the sequel to a special
treatment. It may be useful to make here the remark that
the theory just given is equivalent to supposing that, instead

of the expression originally given for Δu, we use the second approximation,

$$\Delta u = \left(h \frac{d}{dx} + k \frac{d}{dy} + l \frac{d}{dz} \right) u + \frac{1}{2} \left(h \frac{d}{dx} + k \frac{d}{dy} + l \frac{d}{dz} \right)^2 u.$$

But, to pass on, the state of strain of the element depends now upon the following groups of magnitudes:—

(I.)   A, B, C, D, E, F ;

(II.) ϖ_1, ϖ_2, ϖ_3 ;

(III.) a, b, c, d, e, f ;

(IV.) ϖ'_1, ϖ'_2, ϖ'_3 ;

(V.) $\dfrac{dA}{dx}$, $\dfrac{dA}{dy}$, $\cdots \dfrac{dF}{dz}$.

We may now follow Green, and assume that the energy in the element-volume can depend only on these five groups. It must, however, be independent of group II.; for if any term contained ϖ_1, the energy in the substance might be increased or diminished by giving it a rotation as a whole round the axis of x—a result contrary to experience. It must also be remembered that the constituents of the last three groups bring with them coefficients involving as a factor the intermolecular distance $\mathfrak{m}$; and this will cause the coefficients of these terms to be very small compared with those which arise from the combination of group I. with itself.

Designating, therefore, as of class (I., III.) those terms which arise from the combination of groups I. and III., we need only consider the classes (I., I.), (I., III.), (I., IV.), (I., V.) and neglect the classes (III., IV.), (III., V.), (IV., V.), the terms of which would involve, in the case of transmission of a plane wave of length λ, the factor $\left(\dfrac{\mathfrak{m}}{\lambda} \right)^2$. We shall thus obtain a first approximation to the theory of a hetero-homogeneous solid.

Specification of the Internal Stress.

4. In the case of a perfectly homogeneous elastic solid, the stresses upon the faces of a rectangular element which intersect at a solid angle are resolvable, in the first place, into nine forces, S_{xx}, S_{xy}, $\ldots S_{zz}$, wherein S_{uv} is the force per unit area on the face perpendicular to the axis of u acting in the direction of the axis of v, and reckoned as tractions exerted by the surrounding substances. And in order that the angular accelerations of the element may be all finite, it is necessary that

the tangential stresses be equal in pairs, so that

$$S_{yz}=S_{zy}, \quad S_{zx}=S_{xz}, \quad S_{xy}=S_{yx}. \quad . \quad . \quad . \quad (4)$$

But in the case of an imperfectly homogeneous solid this specification is no longer sufficient; the stress will involve other elements, depending on the form of the expression for the energy of the solid due to distortion. If, for example, the energy contains terms of classes (I., III.), (I., IV.), it is enough to introduce nine couples of the form $L_{xx} \dots L_{zz}$, where L_{uv} is the couple per unit area on the face of the element perpendicular to the axis of u, the axis of the couple having the direction of the axis of v; and we must also suppose that the tangential stresses are no longer connected by the relations (4). If we suppose the energy to contain terms of the class (I., V.), we have also to introduce force of a different type. We shall return to this point; but in the mean time it may be proved that, in order that the angular accelerations of the element may not be infinite, we must have three new relations connecting the stress-forces and couples, as follows. The symbols $L_{xx} \dots L_{yz}$ referring to the couples on indefinitely small areas placed at the centre of the element, we may suppose that two faces perpendicular to the axis of x are acted on by couples whose axes are in this direction, of amounts equal to

$$\left(L_{xx}+\frac{dx}{2}\cdot\frac{dL_{xx}}{dx}\right)dy\,dz, \quad -\left(L_{xx}-\frac{dx}{2}\cdot\frac{dL_{xx}}{dx}\right)dy\,dz,$$

the resultant of which is the couple

$$\frac{dL_{xx}}{dx}dx\,dy\,dz.$$

In the same way the couples on the other faces will result in two couples, whose axes are in the direction of the axis of x, of amounts

$$\frac{dL_{yx}}{dy}dx\,dy\,dz, \quad \frac{dL_{zx}}{dz}dx\,dy\,dz.$$

We may similarly find the resultant couples in the directions of the axes of y and z. But the stress-forces acting on the faces give rise to the three couples

$$(S_{yz}-S_{zy})dx\,dy\,dz, \quad (S_{zx}-S_{xz})dx\,dy\,dz, \quad (S_{xy}-S_{yx})dx\,dy\,dz.$$

Hence the condition for finite angular acceleration requires that

$$\left.\begin{array}{l} \dfrac{d\mathrm{L}_{xx}}{dx} + \dfrac{d\mathrm{L}_{yx}}{dy} + \dfrac{d\mathrm{L}_{zx}}{dz} + \mathrm{S}_{yz} - \mathrm{S}_{zy} = 0, \\[2mm] \dfrac{d\mathrm{L}_{xy}}{dx} + \dfrac{d\mathrm{L}_{yy}}{dy} + \dfrac{d\mathrm{L}_{zy}}{dz} + \mathrm{S}_{zx} - \mathrm{S}_{xz} = 0, \\[2mm] \dfrac{d\mathrm{L}_{xz}}{dx} + \dfrac{d\mathrm{L}_{yz}}{dy} + \dfrac{d\mathrm{L}_{zz}}{dz} + \mathrm{S}_{xy} - \mathrm{S}_{yx} = 0. \end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

The existence of these internal stress-couples was indicated by Professor Stokes in his review of MacCullagh's theory of double refraction (Rep. Brit. Assoc. 1862).

5. The laws of resolution of these internal forces and couples may be readily investigated by considering the equilibrium of an elementary tetrahedron, three adjacent edges of which are dx, dy, dz. If X, Y, Z be the three forces, and L, M, N the three couples which act on the oblique face per unit area of the same, we shall have

$$\left.\begin{array}{lll} \mathrm{X} = l\mathrm{S}_{xx} + m\mathrm{S}_{yx} + n\mathrm{S}_{zx}, & \mathrm{Y} = l\mathrm{S}_{xy} + m\mathrm{S}_{yy} + n\mathrm{S}_{zy}, & \mathrm{Z} = \dots, \\[2mm] \mathrm{L} = l\mathrm{L}_{xx} + m\mathrm{L}_{yx} + n\mathrm{L}_{zx}, & \mathrm{M} = l\mathrm{L}_{xy} + \dots, & \mathrm{N} = \dots, \end{array}\right\} (6)$$

l, m, n being the direction-cosines of the normal to the oblique face. These expressions satisfy the condition that the work done by the external forces on the oblique face ($d\Sigma$) during any indefinitely small arbitrary twist ($\delta u, \delta v, \delta w, \delta \varpi_1, \delta \varpi_2, \delta \varpi_3$) shall be

$$d\Sigma(\mathrm{X}\delta u + \mathrm{Y}\delta v + \mathrm{Z}\delta w + \mathrm{L}\delta\varpi_1 + \mathrm{M}\delta\varpi_2 + \mathrm{N}\delta\varpi_3); \quad \cdot \quad \cdot \quad (7)$$

and, indeed, the values of X, Y, ... N might have been found from this condition by the application of the principle of virtual velocities.

6. We shall now prove that, if the energy contain terms belonging to either of the classes (I., III.) or (I., IV.), the corresponding part of the stress will consist of a force and couple satisfying equations (5). Let us consider any portion of the solid, and let it receive infinitesimal arbitrary displacements throughout its interior and over its boundary, and let $\delta \mathrm{W}$ be the work done by the forces arising from the action of the surrounding matter, and $\delta \mathrm{E}$ the increase of energy stored up in the solid; the increase of kinetic energy $\delta\kappa$ will be

$$\iiint\left(\frac{d^2u}{dt^2}\,\delta u + \frac{d^2v}{dt^2}\,\delta v + \frac{d^2w}{dt^2}\,\delta w\right) dx\,dy\,dz,$$

the density being taken as unity for simplicity, and impressed forces neglected for the same reason. By the principle of the conservation of energy we have

$$\delta\mathrm{W} = \delta\mathrm{E} + \delta\kappa; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (8)$$

and if δE be now separated by integration into surface-terms and a term of the form $\iiint (P\delta u + Q\delta v + R\delta w)dx\,dy\,dz$, we may find at once both the components of the stress over the boundary and the equations of motion.

If we consider the term in E due to $A\dfrac{d\varpi_1}{dy}$, the result may be most conveniently exhibited thus:—Let $d\Sigma$ be any element of the surface, $l\,m\,n$ the direction-cosines of the normal at that point, and let $\int_\Sigma$, $\int_V$ denote integrations over the surface and throughout the enclosed solid respectively ; then

$$\delta . \int_V A\frac{d\varpi_1}{dy} = \int_\Sigma \left(Am\delta\varpi_1 + \frac{d\varpi_1}{dy} . l\delta u + \tfrac{1}{2}\frac{dA}{dy} . n\delta v - \tfrac{1}{2}\frac{dA}{dy}m\delta w \right)$$
$$+ \int_V \left(-\frac{d^2\varpi_1}{dx\,dy}\delta u - \tfrac{1}{2}\frac{d^2A}{dy\,dz}\delta v + \tfrac{1}{2}\frac{d^2A}{dy^2}\delta w \right).$$

The corresponding parts of the stresses are

$$S_{xx} = \frac{d\varpi_1}{dy}, \quad S_{zy} = \tfrac{1}{2}\frac{dA}{dy}, \quad S_{yz} = -\tfrac{1}{2}\frac{dA}{dy}, \quad L_{yx} = A,$$

the others being zero.

These evidently satisfy equations (5); and the same thing may be proved true for each of the terms arising from multiplying $A, B, \ldots F$ by differential coefficients of $\varpi_1, \varpi_2, \varpi_3$.

Theory of an Isotropic Solid.

7. We shall now find the form of the energy when the substance is isotropic. This is done by considering what invariants arise by combining the different groups with group I.

(1) Group I. gives of itself the two invariants,

$$A + B + C, \quad D^2 + E^2 + F^2 - BC - CA - AB.$$

(2) Group III. gives the invariant $a+b+c$, which, however, is identically zero. Class (I., III.) contains the invariant

$$Aa + Bb + Cc + 2Dd + 2Ee + 2Ff.$$

(3) There is no invariant in class (I., IV.), and none in class (I.,V.); for $\varpi'_1 \ldots$ are cogredient with xyz, and $\dfrac{dA}{dx} \ldots \dfrac{dF}{dz}$ are cogredient with $\xi x^2 \ldots \zeta xy$.

The energy corresponding to the element-volume is therefore equal to

$$\{\lambda(A + B + C)^2 + \mu(D^2 + E^2 + F^2 - BC - CA - AB)$$
$$+ \nu(Aa + Bb + \ldots 2Ff)\}dx\,dy\,dz. \quad . \quad (9)$$

We might now find, by the method of last article, the values of the stress-components and the equations of motion of the solid. If this were done, it would be found that the substance transmits in every direction two circular waves oppositely polarized, and would therefore give rise to rotatory polarization. We need not stay to discuss these calculations, as the results are included under the general theory of an æolotropic solid, to which I now proceed.

Theory of an Æolotropic Solid.

8. To the terms which occur in the ordinary theory of a homogeneous substance we have now to add the terms of classes (I., III.), (I., IV.), (I., V.). This part of the energy apparently contains $6 \times 27 = 162$ coefficients. But it must be noted that there are nine relations connecting the members of group V. with the differential coefficients of ϖ_1, ϖ_2, ϖ_3, and that these differential coefficients are also connected by the relation

$$\frac{d\varpi_1}{dx} + \frac{d\varpi_2}{dy} + \frac{d\varpi_3}{dz} = 0.$$

The number of independent constants is thus reduced to 102. I shall now endeavour to ascertain whether these terms afford any illustration of rotatory polarization in crystals, as they do in isotropic substances. It must be admitted that, in the present state of science, theories of double refraction based on the study of the vibrations of elastic bodies are rather of the nature of dynamical illustrations than real explanations. Still, even if they serve no more useful end, they may help us to picture to ourselves the geometrical laws of the phenomena. Green has given two theories of double refraction, one of which was based upon the hypothesis that the elastic substance was primitively free from strain, and transmitted in every direction two plane waves whose vibrations were strictly in the front of the wave. In his second theory he introduces an initial state of stress, and by properly determining these stresses obtains the same geometrical construction for the wave-velocities as before, but with the direction of vibration now at right angles to that in the former theory. I shall make use of the first theory, and apply the same hypothesis of two strictly transversal waves to the new terms now introduced. It is clear, however, that the results will not be affected by adopting the second form of Green's theory.

The following method of introducing the condition that the substance shall always transmit two strictly transversal waves is substantially the same as that given by Lamé in his repro-

duction of Green's theory, and is somewhat more easily applied than his process or Green's.   Let the small motions of a solid be given by the equations

$$\frac{d^2u}{dt^2}=f_1\left(\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dv}\right)(u, v, w), \quad \frac{d^2v}{dt^2}=f_2\left(\frac{d}{dx}, \dots\right)(u,v,w), \dots;$$

then, if there be two transversal waves in every direction, the expression for $\frac{d^2\theta}{dt^2}$ or $\frac{d^2}{dt^2}\left(\frac{du}{dv} + \frac{dv}{dy} + \frac{dw}{dz}\right)$ must be of the form

$$\phi\left(\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}\right)\theta.$$

For let us consider, first of all, an ordinary homogeneous solid, for which f_1, f_2, f_3 are quadratic functions of $\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}$. We may satisfy the equations by putting $u, v, w = (u_0, v_0, w_0)\sin\zeta$, where $\zeta = \frac{2\pi}{\lambda}(lx + my + nz - \mathrm{V}t)$, obtaining the three following equations,

$$u_0, v_0, w_0 . \mathrm{V}^2 = f_1, f_2, f_3 . (l, m, n)(u_0, v_0, w_0).$$

But if $\mathrm{V}^2(u_0 l + v_0 m + w_0 n)$, derived from these, do not contain $u_0 l + v_0 m + w_0 n$ as a factor, we shall have two equations of the form $u_0 l + v_0 m + w_0 n = 0$, $u_0 l' + v_0 m' + w_0 n' = 0$, giving a single value for the ratios $u_0 : v_0 : w_0$, and by consequence a single value of V^2, contrary to the hypothesis.

The equations of motion of the solid take the form, as Lamé has shown,

$$\left.\begin{array}{l}\dfrac{d^2u}{dt^2} = 2b_1^2\dfrac{d\varpi_2}{dz} - 2c_1^2\dfrac{d\varpi_3}{dy}, \\[2mm] \dfrac{d^2v}{dt^2} = 2c_1^2\dfrac{d\varpi_3}{dx} - 2a_1^2\dfrac{d\varpi_1}{dz}, \\[2mm] \dfrac{d^2w}{dt^2} = 2a_1^2\dfrac{d\varpi_1}{dy} - 2b_1^2\dfrac{d\varpi_2}{dx}, \end{array}\right\} \quad \dots \quad (10)$$

wherein a_1, b_1, c_1 are the three principal wave-velocities.

In the case where f_1, f_2, f_3 contain third as well as second powers of $\frac{d}{dx}\dots$, the necessary condition is clearly fulfilled by supposing the principle stated to apply to both sets of terms; and it may be readily shown that this is the only conclusion which will satisfy it.

Reduction of terms of Class (I., III.).

9. The part of the energy arising from this source will be

of the form $\int_V J$, where

$$J = K_{11}Aa + K_{12}Ab + \ldots + K_{66}Ff,$$

$K_{11} \ldots K_{66}$ being constants. The variation of J,

$$\delta J = K_{11}(A\delta a + a\delta A) + \ldots;$$

and each of the terms of this sum may be separately treated by the method given in art. 6, and applied to find the corresponding parts of the stress-components and of the equations of motion, and thence the value of $\frac{d^2\theta}{dt^2}$. But we may avoid much labour in finding $\frac{d^2\theta}{dt^2}$, if we observe that the variation of δa can contribute nothing towards it. For

$$\int_V A\delta a = 2\int_\Sigma lA - 2\int_V \delta\varpi_1 \frac{dA}{dx};$$

and we may put

$$-2\delta\varpi_1 = \delta\left(\frac{dv}{dz} - \frac{dw}{dy}\right),$$

which is equal to

$$\frac{d\delta v}{dz} - \frac{d\delta w}{dy}.$$

Thus, on integrating by parts again, we see that $\int_V A\delta a$ contains, besides surface-integrals, the volume-integral

$$\iiint \left(\delta v \cdot \frac{d^2A}{dx\,dz} - \delta w \frac{d^2A}{d\tilde{x}\,dy}\right) dx\,dy\,dz.$$

Hence it appears that the part which this term contributes to $\frac{d^2v}{dt^2}$ is $-K_{11}\frac{d^2A}{dx\,dz}$, and the part contributed to $\frac{d^2w}{dt^2}$ is $K_{11}\frac{d^2A}{dx\,dy}$. Taken together they contribute nothing to $\frac{d^2\theta}{dt^2}$. The same thing is true for the terms due to δb, δc, δd, δe, δf.

Writing, therefore, for A, B, ... F their values, we find that

$$\int_V \delta J = \int_V \left(K_{11}a\frac{d\delta u}{dx} + \ldots + \tfrac{1}{2}K_{41}a\left(\frac{d\delta w}{dy} + \frac{d\delta v}{dz}\right) + \ldots\right).$$

Integrating by parts, and forming the equations in $\frac{d^2u}{dt^2}, \ldots$, we obtain the following equation in $\frac{d^2\theta}{dt^2}$,

$$\frac{d^2\theta}{dt^2} = \frac{d^2}{dx^2}\{K_{11}a + K_{12}b + \ldots + K_{16}f\}$$

$$+ \ldots$$

$$+ \frac{d^2}{dx\,dy}\{K_{61}a + K_{62}b + \ldots + K_{66}f\}.$$

Now, according to the test given above, the right-hand member of this equation ought to contain differential coefficients of θ only; but inasmuch as u, v, w appear only as contained in ϖ_1, ϖ_2, ϖ_3, and as $\dfrac{d\varpi_1}{dx} + \dfrac{d\varpi_2}{dy} + \dfrac{d\varpi_3}{dz} = 0$, it is obvious that the right-hand member contains differential coefficients of $\dfrac{d\varpi_1}{dx} + \dfrac{d\varpi_2}{dy} + \dfrac{d\varpi_3}{dz}$ only. Writing, therefore, for $a, b, \ldots f$ their values, we have

$$\frac{d^2}{dx^2}\left\{ K_{11} \cdot 2\frac{d\varpi_1}{dx} + K_{12} \cdot 2\frac{d\varpi_2}{dy} + \ldots + K_{16}\left(\frac{d\varpi_2}{dx} + \frac{d\varpi_1}{dy}\right) \right\}$$

$$+ \ldots$$

$$+ \frac{d^2}{dx\,dy}\left\{ K_{61} \cdot 2\frac{d\varpi_1}{dx} + \ldots\ldots\ldots + K_{66}\left(\frac{d\varpi_2}{dx} + \frac{d\varpi_1}{dy}\right) \right\}$$

$$= \left(K_1\frac{d^2}{dx^2} + K_2 \cdot \frac{d^2}{dy^2} + \ldots + 2K_6\frac{d^2}{dx\,dy} \right)\left(\frac{d\varpi_1}{dx} + \frac{d\varpi_2}{dy} + \frac{d\varpi_3}{dz}\right). \quad (11)$$

A comparison of the coefficients of like terms on both sides of this equation readily furnishes the following relations among the coefficients of J,

$$0 = K_{15} = K_{16} = K_{24} = K_{26} = K_{34} = K_{35};$$

$$K_{56} = K_{65} = -K_{14}, \quad K_{46} = K_{64} = -K_{25}, \quad K_{45} = K_{54} = -K_{36};$$

$$2K_1 = 2K_{11} = 2K_{12} + K_{66} = 2K_{13} + K_{55},$$

$$2K_2 = 2K_{22} = 2K_{21} + K_{66} = 2K_{23} + K_{44},$$

$$2K_3 = 2K_{33} = 2K_{31} + K_{55} = 2K_{32} + K_{44},$$

$$K_4 = K_{42} = K_{43} = K_{41} - K_{14},$$

$$K_5 = K_{51} = K_{53} = K_{52} - K_{25},$$

$$K_6 = K_{61} = K_{62} = K_{63} - K_{36}.$$

Substituting these relations in J, we obtain

$$\begin{aligned} J = {} &(K_1A + K_2B + K_3C + K_4D + K_5E + K_6F)(a+b+c) \\ &- \tfrac{1}{2}K_{44}(Bc + Cb - 2Dd) - \tfrac{1}{2}K_{55}(Ca + Ac - 2Ee) \\ &- \tfrac{1}{2}K_{66}(Ab + Ba - 2Ff) \\ &- K_{14}(Ef + Fe - Ad - Da) - K_{25}(Fd + Df - Be - Eb) \\ &- K_{36}(De + Ed - Cf - Fc). \end{aligned} \quad (12)$$

But this equation may be greatly simplified; for, in the first place, $a + b + c = 0$, and, moreover, $Bc + Cb - 2Dd, \ldots,$ $De + Ed - Cf - Fc$ are cogredient with $X^2, Y^2, \ldots XY$, where $X = y\zeta - z\eta$, $Y = z\xi - x\zeta$, $Z = x\eta - y\xi$, and where $A \ldots F$ are cogredient with $x^2 \ldots xy$, and $a \ldots f$ with $\xi^2 \ldots \xi\eta$. But X, Y, Z are themselves cogredient with lines, being the compo-

nents of the vector product of (x, y, z) (ξ, η, ζ); hence $X^2 \ldots XY$ are cogredient with $x^2 \ldots xy$, and consequently the coefficients $\frac{1}{2}K_{44} \ldots K_{36}$ are also cogredient with $x^2 \ldots xy$. It is possible, therefore, so to choose the axes of coordinates that the last three terms shall vanish; and the expression of J will then reduce to

$$J = -\alpha(Bc + Cb - 2Dd) - \beta(Ca + Ac - 2Ee)$$
$$- \gamma(Ab + Ba - 2Ff). \quad (13)$$

Suppose, then, that the axes are so chosen, and let us determine the values of the accelerations $\frac{d^2u}{dt^2}, \ldots$ due to J, commencing with the variation of the coefficient of γ; we have to put therein

$$A = \frac{du}{dx}, \quad B = \frac{dv}{dy}, \quad F = \tfrac{1}{2}\left(\frac{dv}{dx} + \frac{du}{dy}\right),$$

$$a = 2\frac{d\varpi_1}{dx} = \frac{d^2w}{dx\,dy} - \frac{d^2v}{dx\,dz}, \quad b = \frac{d^2u}{dy\,dz} - \frac{d^2w}{dx\,dy},$$

$$f = \frac{d^2u}{dx\,dz} - \frac{d^2w}{dx^2} + \frac{d^2w}{dy^2} - \frac{d^2v}{dy\,dz}.$$

On substituting these values and integrating properly, we finally obtain for $\delta(Ab + Ba - 2Ff)$ a series of surface-terms with the addition of

$$-\int_V \left\{ \delta u\left(\frac{db}{dx} - \frac{df}{dy} - \frac{d^2A}{dy\,dz} + \frac{d^2F}{dx\,dz}\right) + \delta v\left(\frac{da}{dy} - \frac{df}{dx} + \frac{d^2B}{dx\,dz} - \frac{d^2f}{dy\,dz}\right) \right.$$
$$\left. + \delta w\left(-\frac{d^2B}{dx\,dy} + \frac{d^2A}{dx\,dy} - \frac{d^2F}{dx^2} + \frac{d^2F}{dy^2}\right) \right\}. \quad (14)$$

But

$$-\frac{dA}{dy} + \frac{dF}{dx} = -\frac{d^2u}{dx\,dy} + \tfrac{1}{2}\frac{d}{dx}\left(\frac{du}{dy} + \frac{dv}{dx}\right) = \frac{d\varpi_3}{dx},$$

$$\frac{dB}{dx} - \frac{dF}{dy} = \frac{d^2v}{dx\,dy} - \tfrac{1}{2}\frac{d}{dy}\left(\frac{du}{dy} + \frac{dv}{dx}\right) = \frac{d\varpi_3}{dy}.$$

Substituting these, and inserting for a, b, f their proper values, expression (14) may be reduced to

$$+\int_V \left\{ \delta u\left(\frac{d^2\varpi_1}{dx^2} + \frac{d^2\varpi_1}{dy^2}\right) + \delta v\left(\frac{d^2\varpi_2}{dx^2} + \frac{d^2\varpi_2}{dy^2}\right) \right.$$
$$\left. + \delta w\left(\frac{d^2\varpi_3}{dx^2} + \frac{d^2\varpi_3}{dy^2}\right) \right\}. \quad (15)$$

We may now take together all the three terms of J in (13), when we shall clearly obtain for the three accelerations $\partial^2\varpi_1$,

$\partial^2\varpi_2, \partial^2\varpi_3$, where

$$\partial^2 = \alpha\left(\frac{d^2}{dy^2} + \frac{d^2}{dz^2}\right) + \beta\left(\frac{d^2}{dz^2} + \frac{d^2}{dx^2}\right) + \gamma\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2}\right). \quad (16)$$

The acceleration in any other definite direction is obviously $\partial^2\varpi^1$, where ϖ^1 is the component rotation in that direction. Consequently the component accelerations along the original axes are

$$\partial^2\varpi_1, \ \partial^2\varpi_2, \ \partial^2\varpi_3,$$

where

$$\partial^2 = a_2\frac{d^2}{dx^2} + b_2\frac{d^2}{dy^2} + c_2\frac{d^2}{dz^2} + \ldots + 2f_2\frac{d^2}{dx\,dy}, \quad (17)$$

some quadratic function of $\dfrac{d}{dx}, \dfrac{d}{dy}, \dfrac{d}{dz}$.

Adding these terms to the equations (10), the equations of motion of the solid now become

$$\left.\begin{aligned}
\frac{d^2u}{dt^2} &= 2b_1^2\frac{d\varpi_2}{dz} - 2c_1^2\frac{d\varpi_3}{dy} + \partial^2\varpi_1, \\
\frac{d^2v}{dt^2} &= 2c_1^2\frac{d\varpi_3}{dx} - 2a_1^2\frac{d\varpi_1}{dz} + \partial^2\varpi_2, \\
\frac{d^2w}{dt} &= 2a_1^2\frac{d\varpi_1}{dy} - 2b_1^2\frac{d\varpi_2}{dx} + \partial^2\varpi_3.
\end{aligned}\right\} \quad . \quad . \quad (18)$$

I shall return to the solution of these in art. (12), and in the mean time proceed to the discussion of the

Terms of Class (I., IV.).

10. There is no reason *à priori* why the group $\varpi_1', \varpi_2', \varpi_3'$ should not appear in the expression for the energy of the solid; for they correspond to a real strain of it. This species of strain may be easily analyzed geometrically, and has some interesting properties, especially in an isotropic homogeneous solid. Let us pass, however, to the expression for the energy depending on these terms, which must be of the form

$$\int_V \{\varpi_1'(N_{11}A + N_{12}B + \ldots + N_{16}F) + \varpi_2'(N_{21}A + \ldots + N_{26}F)$$
$$+ \varpi_3'(N_{31}A + \ldots + N_{36}F)\}.$$

The value of $\dfrac{d^2\theta}{dt^2}$ which corresponds to it is clearly

$$\left(N_{11}\frac{d^2}{dx^2} + \ldots + N_{16}\frac{d^2}{dx\,dy}\right)\varpi_1' + \left(N_{21}\frac{d^2}{dx^2} + \ldots + N_{26}\frac{d^2}{dx\,dy}\right)\varpi_2'$$
$$+ \left(N_{31}\frac{d^2}{dx^2} + \ldots\right)\varpi_3'.$$

But since

$$\frac{d\varpi_1}{dx} + \frac{d\varpi_2'}{dy} + \frac{d\varpi_3'}{dz} = 0,$$

this must be (to admit of two transversal waves) of the form

$$\left(N_1\frac{d}{dx} + N_2\frac{d}{dy} + N_3\frac{d}{dz}\right)\left(\frac{d\varpi_1'}{dx} + \frac{d\varpi_2}{dy} + \frac{d\varpi_3'}{dz}\right),$$

whence the following series of relations:—

$$0 = N_{12} = N_{13} = N_{14} = N_{21} = N_{23} = N_{25} = N_{31} = N_{32} = N_{36},$$
$$N_1 = N_{11} = N_{26} = N_{35}, N_2 = N_{22} = N_{16} = N_{34}, N_3 = N_{15} = N_{24} = N_{33};$$

and the energy takes the form

$$\int_V \{N_1(A\varpi_1' + F\varpi_2' + E\varpi_3'') + N_2(F\varpi' + B\varpi_2' + D\varpi_3')$$
$$+ N_3(E\varpi_1' + D\varpi_2' + C\varpi_3')\}. \quad (19)$$

Now A ... F are cogredient with $x^2 \ldots xy$, and ϖ_1', ϖ_2', ϖ_3' with ξ, η, ζ; thus the coefficients of N_1, N_2, N_3 are cogredient with $x(\xi x + \eta y + \zeta z), y(\xi x + \eta y + \zeta z), z(\xi x + \eta y + \zeta z)$—that is to say, with lines. The energy in an element of volume appears, therefore, of the nature of the component of a force along a line, like the potential of a small magnet in a magnetic field. It may be shown, however, that the energy given by (19) resides wholly at the surface of the body. In fact, let

$$\lambda = m\varpi_3 - n\varpi_2, \quad \mu = n\varpi_1 - l\varpi_3, \quad \nu = l\varpi_2 - m\varpi_1,$$

and let $\varpi^2 = \varpi_1^2 + \varpi_2^2 + \varpi_3^2$; then the total energy of the solid arising from terms of this class is

$$\int_\Sigma \{N_1(A\lambda + F\mu + E\nu + \tfrac{1}{2}l\varpi^2) + N_2(F\lambda + B\mu + D\nu + \tfrac{1}{2}m\varpi^2)$$
$$+ N_3(E\lambda + D\mu + C\nu + \tfrac{1}{2}n\varpi^2)\}. \quad (20)$$

Such terms therefore, if they existed, could not affect the internal motions of the solid.

Terms of Class (I., V.).

11. We now come to the terms formed by the combination of groups I., V. Some of these are already complete integrals, such as $A\dfrac{dA}{dy}$, the integral of which throughout the solid $= \tfrac{1}{2}\int_\Sigma mA^2$. Moreover it can be shown that, in every case, any term of the class (I., V.) may be resolved into a series of such surface-terms coupled with terms of classes (I., III.), (I., IV.). The proof of this proposition will be given

for two of these terms; and we may observe that it follows from this that the interior parts of these terms may be supposed to have been already combined with those belonging directly to classes (I., III.), (I., IV.). The existence, therefore, of the terms of the class at present under consideration in no way affects the internal movements of the solid, though it does affect the specification of the stress to which the solid is everywhere subject. For in addition to the forces and couples which have been already considered in art. 6, we must also imagine stress whose proximate effect is a variation in the state of strain of the parts of the solid adjacent to the plane at which it acts. I proceed now to prove the proposition mentioned in this article by taking two terms of the class, viz. $\int_V A\dfrac{dB}{dz}$ and $\int_V A\dfrac{dF}{dz}$. It is unnecessary to trouble the reader with the details of the reduction; and I shall therefore merely state that the former of these integrals is equal to

$$\int_\Sigma \left(-DFl + ADm + \frac{F^2}{2}n\right) + \int_V \left(-A\frac{d\varpi_1}{dy} - F\frac{d\varpi_2}{dy} + D\frac{d\varpi_3}{dx}\right),$$

and the latter to

$$\tfrac{1}{2}\int_\Sigma (-EFl + AEm + AFn) + \tfrac{1}{2}\int_V \left(A\frac{d\varpi_3}{dy} + E\frac{d\varpi_3}{dx} - F\frac{d\varpi_2}{dx}\right).$$

Each of the other terms of this class may be treated in the same manner; and we may therefore consider the proposition stated above to be established, and proceed to the consideration of the equations (18).

Solution of the Equations of Motion.

12. Before proceeding to the general equations (18) it will be convenient to consider, first of all, equations (10). By differentiation we obtain from these latter the following:—

$$\frac{d^2\varpi_1}{dt^2} = a_1^2\nabla^2\varpi_1 - \frac{d\vartheta}{dx},$$

$$\frac{d^2\varpi_2}{dt^2} = b_1^2\nabla^2\varpi_2 - \frac{d\vartheta}{dy},$$

$$\frac{d^2\varpi_3}{dt^2} = c_1^2\nabla^2\varpi_3 - \frac{d\vartheta}{dz},$$

where

$$\nabla^2 = \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2},$$

$$\vartheta = a_1^2\frac{d\varpi_1}{dx} + b_1^2\frac{d\varpi_2}{dy} + c_1^2\frac{d\varpi_3}{dz}.$$

Now equations (10) may be satisfied by $u, v, w = u_0, v_0, w_0 \cdot \sin \zeta$, where $\zeta = \dfrac{2\pi}{\lambda}(lx + my + nz - \mathrm{V}t)$. If we find by differentiation $\varpi_1, \varpi_2, \varpi_3$, it is easily seen that they form a vector at right angles to $u_0\, v_0\, w_0$, and to $l\, m\, n$. If therefore its components be $\mathrm{U}_0, \mathrm{V}_0, \mathrm{W}_0 \cdot \cos \zeta$, we shall have

$$
\left.
\begin{aligned}
l\mathrm{U}_0 + m\mathrm{V}_0 + n\mathrm{W}_0 &= 0, \\
(\mathrm{V}^2 - a_1^2)\mathrm{U}_0 &= -l\vartheta_0, \\
(\mathrm{V}^2 - b_1^2)\mathrm{V}_0 &= -m\vartheta_0, \\
(\mathrm{V}^2 - c_1^2)\mathrm{W}_0 &= -n\vartheta_0,
\end{aligned}
\right\} \quad . \quad . \quad . \quad (21)
$$

where

$$
\vartheta_0 = a_1^2 l\mathrm{U}_0 + b_1^2 m\mathrm{V}_0 + c_1^2 n\mathrm{W}_0.
$$

The two values of V are therefore the reciprocals of the semiaxes of the section of $a^2x^2 + b^2y^2 + c^2z^2 = 1$ by the plane $lx + my + nz = 0$, and the corresponding resultant rotations are in the directions of these axes respectively. It follows from this that if l_1, m_1, n_1 be the direction-cosines of the semiaxis V_1^{-1}, and if from equations (21) we determine the value of

$$
\mathrm{V}^2(l_1\mathrm{U}_0 + m_1\mathrm{V}_0 + n_1\mathrm{W}_0),
$$

this can be nothing else than

$$
\mathrm{V}_1^2(l_1\mathrm{U}_0 + m_1\mathrm{V}_0 + n_1\mathrm{W}_0),
$$

whence the following *Lemma*,

$$
(a_1^2\mathrm{U}_0 - l\vartheta_0)l_1 + (b_1^2\mathrm{V}_0 - m\vartheta_0)n_1 + (c_1^2\mathrm{W}_0 - n\vartheta)n_1
$$
$$
= \mathrm{V}_1^2(l_1\mathrm{U}_0 + m_1\mathrm{V}_0 + n_1\mathrm{W}_0); \quad . \quad (22)
$$

which may be also verified directly with the greatest ease, remembering that $ll_1 + mm_1 + nn_1 = 0$, and that

$$
l_1 : m_1 : n_1 :: \frac{l}{\mathrm{V}_1^2 - a_1^2} : \frac{m}{\mathrm{V}_1^2 - b_1^2} : \frac{n}{\mathrm{V}_1^2 - c_1^2}.
$$

13. We turn now to the equations (18), by differentiating which we obtain

$$
\frac{d^2\varpi_1}{dt^2} = a_1^2 \nabla^2 \varpi_1 - \frac{d\vartheta}{dx} + \tfrac{1}{2}\eth^2\left(\frac{d\varpi_3}{dy} - \frac{d\varpi_2}{dz}\right),
$$

$$
\frac{d^2\varpi_2}{dt^2} = b_1^2 \nabla^2 \varpi_2 - \frac{d\vartheta}{dy} + \tfrac{1}{2}\eth^2\left(\frac{d\varpi_1}{dz} - \frac{d\varpi_3}{dx}\right),
$$

$$
\frac{d^2\varpi_3}{dt^2} = c_1^2 \nabla^2 \varpi_3 - \frac{d\vartheta}{dz} + \tfrac{1}{2}\eth^2\left(\frac{d\varpi_2}{dx} - \frac{d\varpi_1}{dy}\right).
$$

These equations will be satisfied by $\varpi_1, \varpi_2, \varpi_3 = f, g, h \cdot e^{\zeta\sqrt{-1}}$, where

$$\zeta = \frac{2\pi}{\lambda}(lx + my + nz - \mathrm{V}t),$$

and where also

$$lf + mg + nh = 0,$$

$$\left.\begin{array}{l} \mathrm{V}^2 f = a_1^2 f - l\theta + \mu\sqrt{-1}(mh - ng), \\ \mathrm{V}^2 g = b_1^2 g - m\theta + \mu\sqrt{-1}(nf - lh), \\ \mathrm{V}^2 h = c_1^2 h - n\theta + \mu\sqrt{-1}(lg - mf), \end{array}\right\} \quad . \quad . \quad (23)$$

and

$$\theta = a_1^2 lf + b_1^2 mg + c_1^2 nh,$$

$$\mu = \frac{\pi}{\lambda}(a_2 l^2 + b_2 m^2 + \ldots + 2f_2 lm). \quad . \quad . \quad (24)$$

Let us now resolve the left-hand members of (23) along (1) the semiaxis V_1^{-1}, (2) the semiaxis V_2^{-1}, (3) the normal to the wave; that is to say, let us choose these lines as the new axes of x_1, y_1, z_1. The new components of rotation will be (f_1, g_1, o), and the direction-cosines of the normal will be $(0, 0, 1)$; while the known cogrediency with lines of the terms $mh - ng$, $nf - lh$, $lg - mf$ enables us to write down at once the result of transforming these terms. Coupling this observation with the Lemma expressed in equation (22), the result of the transformation of axes will evidently be as follows:—

$$\left.\begin{array}{l} \mathrm{V}^2 f_1 = \mathrm{V}_1^2 f_1 - \mu g_1 \sqrt{-1}, \\ \mathrm{V}^2 g_1 = \mathrm{V}_2^2 g_1 + \mu f_1 \sqrt{-1}, \\ \zeta = \frac{2\pi}{\lambda}(z_1 - \mathrm{V}t). \end{array}\right\} \quad . \quad . \quad . \quad (25)$$

These will be satisfied by taking $f_1 = \mathrm{P}$, $g_1 = -\epsilon\mathrm{P}\sqrt{-1}$, the corresponding parts of the rotation being

$$\mathrm{P}(\cos\zeta + \sqrt{-1}\sin\zeta), \quad \epsilon\mathrm{P}(\sin\zeta - \sqrt{-1}\cos\zeta).$$

If we consider only the real parts $\mathfrak{F}, \mathfrak{G}$ of these we have

$$\mathfrak{F} = \mathrm{P}\cos\zeta, \quad \mathfrak{G} = \epsilon\mathrm{P}\sin\zeta,$$

which correspond to an elliptic wave. We may evidently produce this real wave by compounding two imaginary ones; and we may thus at once proceed without further remark to the discussion of the imaginary wave. Substituting these

values of f_1 and g_1 in (25), we obtain

$$\left.\begin{array}{l} V^2 - V_1^2 = -\mu\epsilon, \\ V^2 - V_2^2 = -\mu\epsilon^{-1}. \end{array}\right\} \quad \cdots \quad (26)$$

Here, although μ contains λ and by consequence V, the periodic time being supposed known, we may without sensible error treat λ as constant in finding the value of V, which is given by

$$(V^2 - V_1^2)(V^2 - V_2^2) = \mu^2. \quad \cdots \quad (27)$$

Let W_1^2, W_2^2 be the two values of V^2 derived from this equation, and let ϵ_1 and ϵ_2 be the two corresponding values of ϵ; then

$$-\mu\epsilon_1 = W_1^2 - V_1^2,$$
$$-\mu\epsilon_2 = W_2^2 - V_1^2;$$
$$\therefore \mu^2\epsilon_1\epsilon_2 = (V_1^2 - W_1^2)(V_1^2 - W_2^2).$$

But since W_1^2, W_2^2 are the two values of V^2 from (27), we have

$$(V^2 - W_1^2)(V^2 - W_2^2) = (V^2 - V_1^2)(V^2 - V_2^2) - \mu^2,$$
$$\therefore \mu^2\epsilon_1\epsilon_2 = -\mu^2,$$
$$\epsilon_1\epsilon_2 = -1. \quad \cdots \quad (28)$$

The waves, therefore, are oppositely polarized, according to Professor Stokes's definition; that is to say, their eccentricities are equal, their major axes at right angles, and the directions of rotation opposite to each other.

14. It is difficult to verify these conclusions in the case of biaxal crystals, as the rotatory effect is masked by the effects due to double refraction. But in the case of the uniaxal crystal quartz, they coincide with the laws resumed by Airy in explaining the spirals due to it.

For a ray transmitted along the axis of quartz we may put $V_1 = V_2 = c_1$ and $\mu = \dfrac{\pi}{\lambda} c_2$. The equation in V then becomes $(V^2 - c_1^2)^2 = \mu^2$, whence $V^2 = c_1^2 \pm \mu$, and

$$V = c_1\left(1 \pm \frac{\mu}{2c_1^2}\right) \text{ very nearly.}$$

These values of V we shall call W_1 and W_2.

If two waves of the same period T be combined to produce a plane-polarized wave, the rotation of the plane of polarization for thickness z is

$$\rho = \frac{2\pi}{T} \cdot \frac{z}{2}\left(\frac{1}{W_1} \sim \frac{1}{W_2}\right) = \frac{2\pi}{T} \cdot \frac{\mu z}{2c_1^3}.$$

If i be the index of refraction, from air, of light having period T, λ_0 the wave-length in air, U the velocity of light in air,

$$\lambda_0 = U \cdot T, \quad i = \frac{U}{c_1},$$

whence

$$\rho = \frac{\pi i \mu z}{\lambda_0 c_1^2}.$$

If L be the length of solid necessary to produce a complete rotation of the plane of polarization,

$$2\pi = \frac{\pi i \mu L}{\lambda_0 c_1^2},$$

wherein we may replace μ by its value $\frac{\pi}{\lambda} \cdot c_2$.

Sir W. Thomson has shown that the phenomenon of the dispersion of light enables us to form an estimate of the intermolecular distances. But the phenomena of rotatory polarization afford a vastly more delicate test of differences of wave-velocity. In quartz, for example, whose rotatory effect is about $24°$ per millimetre, the rotation of the plane of polarization reveals a difference of velocities amounting to less than the 20,000th part of either. We may therefore hope to find in these data some evidence regarding the distance $\mathfrak{m}$ throughout which the want of homogeneity of structure is sensible.

The magnitude c_2, used in the last equation, and introduced in art. 9, is obviously of the form $\mathfrak{m} \cdot \mathfrak{A}$, where $\mathfrak{A}$ is a quantity of the same dimensions as c_1^2, and may, for aught we know, be of the same order of magnitude. The last equation may therefore be written

$$\frac{\mathfrak{m}}{\lambda} = \left(\frac{c_1^2}{\pi \mathfrak{A}}\right) \cdot \frac{2\lambda_0}{L} \cdot \frac{1}{i}.$$

But for the mean yellow rays of quartz, $\lambda_0 = \frac{1}{2000}$ millim., $L = 15$, $i = \frac{3}{2}$; hence

$$\frac{\mathfrak{m}}{\lambda} = \left(\frac{c_1^2}{\pi \mathfrak{A}}\right) \cdot \frac{1}{22500}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (29)$$

a result apparently in harmony with Sir W. Thomson's conclusions.

XXXIV. *Note on the Polarization of Heat.*
By G. Carey Foster, *F.R.S.*[*]

THE following determinations of the amounts of heat trans-
mitted by two Nicol's prisms, whose principal sections
make different angles with each other, were recently made in
the Physical Laboratory of University College, London, by
Mr. M. J. Jackson. Although the results amount to nothing
more than an additional verification of a relation that is already
thoroughly established, I venture to put them on record, not
only because such verifications are satisfactory in themselves,
but also because the apparatus required for conveniently re-
peating experiments of this kind is not always at hand.

The source of heat was a rather powerful paraffin-oil lamp
(supplied by White, of Glasgow, for use with a Thomson's
quadrant electrometer). The rays from the lamp were con-
centrated by a lens of 7·5 centims. diameter and about 22 cen-
tims. focal length, placed so as to produce a real image of the
lamp-flame within the silvered reflecting cone of the thermo-
pile. Immediately behind the lens (on the side next the lamp)
a double screen of polished sheet brass was placed, whereby
the radiation could be cut off or allowed to pass at will. On the
other side of the lens came two Nicol's prisms, each about 20
centims. long, and giving a clear circular field about 6·7
centims. in diameter. The prisms are protected at the ends
by disks of thin glass, which were left on during the experi-
ments. It is to the possession of these fine prisms, made for
me by Mr. C. D. Ahrens, that the possibility of making the
experiments with so much ease was due. The thermopile,
which was about 95 centims. distant from the lens, was
protected from stray radiation by a double hood of tin-plate.
The galvanometer was a reflecting instrument of low resist-
ance, on Sir William Thomson's principle, made by my assist-
ant, Mr. Grant. By means of a commutator inserted between
the thermopile and the galvanometer, two opposite deflections
were obtained for each position of the prisms. In the follow-
ing Table, the column headed δ_1 gives the means of the deflec-
tions to right and left when the angle, θ, between the prin-
cipal sections of the prisms was measured in one direction;
and the column headed δ_2, the corresponding deflections when
this angle was measured in the opposite direction. The num-
bers denote divisions of the galvanometer-scale.

[*] Communicated by the Physical Society of London, having been read
to the Society on March 3, 1877.

Angle between principal sections $=\theta$.	Deflections.			$\dfrac{\delta}{\cos^2 \theta}$	δ' (calculated).
	δ_1.	δ_2.	Mean δ.		
0°	32·25	31·5	31·9	31·9	31·1
15	29·75	29·25	29·5	31·6	29·0
30	22·75	23·5	23·1	30·8	23·3
45	14·75	14·75	14·75	29·5	15·5
60	7·75	7·75	7·75	31·0	7·8
75	2·25	2·0	2·1	31·7	2·1
90	0·5	0·25	0·4	......	0·0
			Mean ...	31·1	

The numbers in the last column of the Table are calculated by the formula $\delta' = 31 \cdot 1 \cos^2 \theta$. It will be seen that the observed mean values (δ) never differ from the corresponding calculated values (δ') by a whole division of the scale, which represents about as high a degree of accuracy as can be expected from the method of observation employed, it being impossible to read with certainty to any thing less than half a division : where quarter divisions occur in the Table, they result from taking the means of positive and negative deflections.

XXXV. *Physical and Mathematical Principles of the Nebular Theory.* By JACOB ENNIS, *A.M.*[*]

THE chief objections against the derivation of the stars from a former gaseous diffusion of matter have been these two :—first, that no cause can be discovered for the beginning of rotation in the primitive gaseous condition ; and secondly, that this rotation, even if begun, could not become rapid enough to produce a centrifugal equal to the centripetal force, and thus to separate equatorial rings which may break and condense into revolving stars. Both these objections I will now remove.

First. *The force of gravity, by its interaction between nebulous masses, must necessarily begin rotation.*—Sir Isaac Newton says that "if matter were evenly diffused through a finite space and endowed with innate gravity, it would fall down in the middle of that space and form one great spherical mass ; but if matter were diffused through infinite space, some of it would collect into one mass and some into another, so as to form an infinite number of great masses. In this manner the sun and stars might be formed, if the matter were of a lucid nature." This is sound reason, an unavoidable conclusion. Therefore,

given a gaseous diffusion of all matter through all space, and a slow contraction from whatever cause, then there must result an infinite number of separate nebulous masses; and like the clouds in our atmosphere, they must be irregular in shape, different in size, and at unsymmetrical distances apart. Then, by the action of gravity, those which were near must have fallen into each other, until the resulting masses became so far apart as to be beyond each other's sensible gravitation. But when one nebula fell into another it could never fall in the direction of the centre of gravity, because it must have been at the same time under the gravitating influence of other neighbouring nebulæ drawing it from a direct line and causing it to strike obliquely. If we strike a suspended ball in the direction of the centre, it will fly straight onward; but strike it obliquely, and it will spin round. In like manner, from the oblique falls of the nebulæ every resulting mass would rotate.

A small nebula striking a large one very obliquely would cause a rotation only on the surface. Moreover clouds have high prominences, long projecting arms, and extended outliers; all these in falling to the level of rotundity would also be under the influence of other neighbouring nebulæ; and therefore they would fall obliquely and cause surface-currents. These surface-currents, by the composition of forces, would unite in one current; and this one resulting surface-current would be the rotation of the globe. But all surface-currents would be *retarded* by friction on the next interior layers; still the momentum of rotation would not be lost. What was lost by the exterior would be gained by the interior, until the resulting mass rotated, however slowly.

Secondly. *However slowly rotation might begin, the force of gravity would hasten its velocity until on the equatorial zone the centrifugal would equal the centripetal force.*—In consequence of the contraction of the nebulous globe, and of a very slow rotation, a particle at A (fig. 1) would move in the direction from A to C. It would there-fore move in the direction of an inclined plane, and gravity would increase its velocity downwards. All the other particles on the surface represented by dots on the outer circle would also move down in the inclined plane directions toward the centre; and all would be hastened alike by the same force of gravity. There would be no actual inclined planes, for
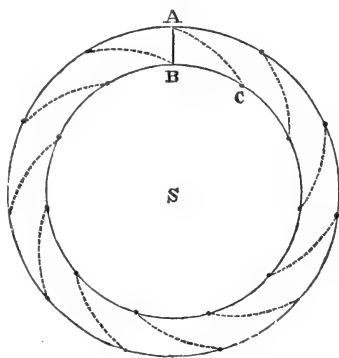
Fig. 1.

every particle would float on the level surface ; still the motions of all the particles would be in the direction of an inclined plane, and subject to calculation as inclined-plane motions.   In fig. 2, a ball rolling down the incline from A to C must gain the same velocity as if it fell from A to B through the same height.   And in fig. 1 the same truth is illustrated by its triangle A B C.   If the outer circle represent the surface of our solar nebula when expanded to the orbit of Neptune, and the inner when expanded to Uranus, then the velocity of rotation gained in contracting from the outer to the inner must be the same as the velocity gained by a fall directly toward the centre from orbit to orbit ; that is, the particle at A moving by rotation and contraction to C would acquire the same velocity as if it fell from A to B,— friction always excepted.

Fig. 2.
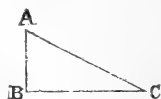
Because action and reaction are equal and in opposite directions, it follows in our terrestrial inclined planes that, as the ball rolls downward and forward, the plane must move upward and backward with the same momentum.   But this upward and backward motion cannot occur in the nebula.   This may be illustrated by the movements of comets.   A comet in its aphelion at A (fig. 3) must gain the same velocity in moving to C as if it fell in a direct line to B, equally near to the sun at S ; the distance B S = C S, and B′ S = C′ S &c. Also when the comet arrives at C′, C″, C‴, and C″″ respectively, it must have the same velocity as if it fell to the points B′, B″, B‴, and B″″ in a direct line, equally near the sun in the focus S. At every step the sun, inversely in proportion to its mass, moves in the direction opposite to that of the comet. But if there were many comets equally large and distant, and moving in the same plane and direction all around the sun, then they would counteract one another's influence on the sun, and the sun would not be moved.   The particles on the surface of the nebula, re-

Fig. 3.
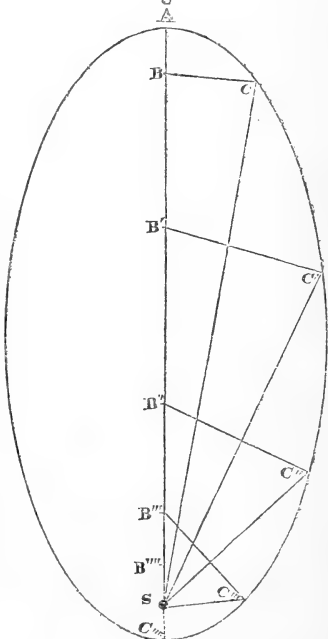
presented by dots on our outer circle (fig. 1) all around, may be regarded as so many comets approaching the sun at the centre S, and they must counteract one another's influence in the same manner. While they move forward and downward more and more rapidly, they produce no upward or backward motion. Moreover they do not, like a rolling ball, press on an inclined surface, but on the level equatorial zone.

Having shown that the particles on the surface of a contracting and rotating nebula must move like a comet approaching the sun with a velocity always accelerated by the force of gravity, let us now attend to the method by which this velocity of rotation may be calculated. Evidently the velocity of rotation gained in contracting from the outer to the inner circle (fig. 1) must be the same as the velocity gained by a fall from one circle to the other in a direct line toward the centre. As gravity, the force causing both the fall and the rotation, varies its power inversely as the squares of the distances, therefore a mass which would be attracted with a power or weight of 409,000,000 pounds at the sun's surface would be attracted with only 9020 pounds at the earth's orbit, with only 10 pounds at the orbit of Neptune, and with only the tenth part of an ounce at forty times the distance of Neptune. We need not inquire how far the ancient nebulæ extended in order to find the beginnings of their falls toward their centres, because the force of gravity was so feeble on their surfaces that we may reject its precise amount, and assume as tantamount all the velocity which could be acquired by a fall from infinite distance toward their centres. If the velocity acquired by a fall from infinite distance to the outer circle be represented by a, and the same to the inner circle be represented by b, then the velocity acquired by a fall from the outer to the inner circle would be $b-a$. The velocity (V) of these falls from infinite distance may be computed by the following formula, $V = \sqrt{2gr}$. Here r stands for the radius of the nebula (of our sun, for instance, when in a nebulous condition), and g stands for the velocity per second acquired by a fall during one second on the surface of the nebula. This velocity on the surface of our nebulous sun may be found from that on our earth's surface, which is 32·16 feet per second, by comparing the mass and radius of our nebulous sun with the mass and radius of our earth. By using this formula, it appears that the velocity of a fall from the orbit of Neptune to that of Uranus becomes a mile and a quarter per second, decimally 1·244. This, added to the actual velocity of Neptune, 3·491, gives a velocity for Uranus of 4·735 miles per second. But the actual velocity of Uranus is 4·369, showing

an excess of ·366. Thus it appears that the force of gravity is enough, and a little more than enough, when acting on a nebulous mass, to cause a rotation with sufficient velocity to make the centrifugal equal to the centripetal force, and therefore to separate nebulous rings from the equatorial zone.—Q. E. D.

But what became of the excess (·366) of the velocity due to the force of gravity? Plainly that proportion of the force was expended by the friction of the rotating exterior layer on the unrotating interior. As I have already shown, the rotation of some of the ancient nebulæ began on the exterior; and therefore the exterior layer must necessarily have been *retarded* by friction on the interior layers. By calculation I have found that this retardation of the velocity due to gravity, all the way from Neptune to Uranus, amounted to 1 per cent. in a radial distance of contraction of 125,000,000 miles; that is, down an inclined plane 125,000,000 miles in height (not in length) the velocity due to gravity was retarded 1 per cent. This retardation became more and more as the unrotating centre of the nebula was approached. The following Table gives the number of miles, not in diameter, but in radial contraction, from planet to planet, necessary to cause a retardation of 1 per cent. in the velocity due to the force of gravity as the sole moving power in the case.

From Neptune to Uranus ...... 125,000,000 miles.
    „   Uranus to Saturn   80,000,000   „
    „   Saturn to Jupiter   38,000,000   „
    „   Jupiter to Mars   13,000,000   „
    „   Mars to Earth   5,783,000   „
    „   Earth to Venus   4,554,000   „
    „   Venus to Mercury   4,459,000   „

This retardation was very small; and the figures show a wonderfully close agreement between the theory and the facts. These facts are vastly multiplied when we take the asteroids into account, as I have done.

As we might anticipate *à priori*, this retardation becomes much larger toward the unrotating centre of the solar nebula. Interior to Mercury it was enormous. At the orbit of Mercury the equatorial surface of the solar nebula rotated with a velocity of 110,000 miles per hour. But it became so greatly retarded by friction on the dense unrotating core, that now the rotation of the solar equator is only 4500 miles per hour. This explains why no planetary ring could be abandoned by centrifugal force interior to Mercury. It also explains why the equatorial region of the sun now makes a rotation in two

or three days shorter time than the polar regions. Those polar regions, being near the axis of rotation, must always have sympathized more with the unrotating interior, because they had less distance to fall to the axis to acquire velocity by their falls. The present excess of equatorial velocity must be regarded as a relic of the past, just as fossil remains in the earth's crust tell of the state of things in times long ago.

It follows from these principles that the atmosphere of the sun should rotate more rapidly than even its liquid equatorial zone; and this has just been discovered to be true by Young. The solar atmosphere must have an inconceivably great momentum; and it cannot easily be stopped by friction on the flaming liquid interior. According to Harkness, " one cubic mile of our atmospheric air weighs 5,621,000 tons." If the solar atmosphere holds the same proportion in mass that ours does to the earth, then its height, according to Trowbridge, must be 606,000 miles. We know of no reason why the process of contraction in the sun should have ceased. And this contraction must add velocity to the solar atmosphere, and daily aid it to overcome friction, and to keep its speed in advance of the underlying liquid body of the sun.

Having shown how rotation must begin in the vast primitive nebulæ which formed sidereal systems, I now proceed to point out how rotation must begin in the nebulæ which resulted from broken rings, and condensed into individual suns and planets and satellites.

Let C (fig. 4) be the centre of a nebula, and A B D an abandoned ring, or fragment of a ring, moving with the arrow. The exterior (*e*) of the ring, or of the globe in which it has condensed, possesses a greater linear velocity than its interior at *n*. Therefore, in contracting, the exterior at *e* will have a greater velocity than the centre, and will fall before it, and the interior at *n* will have a lesser velocity than the centre and will fall behind it. From these two facts, as seen by the arrows moving from *e* and *n*, the beginning of a rotation is plainly a

Fig. 4.

necessity. In the sun we now behold the most unquiet and agitated of all known places, caused, as I have tried to prove, by chemical action. This agitation must have raged with extreme violence in the nebulæ, and must have caused various irregularities in the sizes and densities of the planets, in their distances apart, and inclinations of their axes.
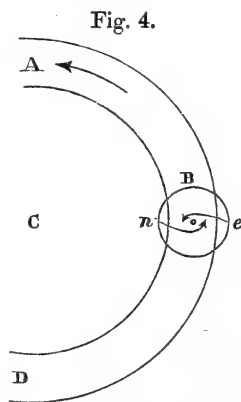
The harmony between gravity as the only projectile force, and the velocities of all the planets and asteroids, is further confirmed by the satellites. By taking into account the masses of the planets in our five planetary systems, I found that in all cases the force of gravity was strong enough to produce in the planetary nebulæ a velocity of rotation such that the centrifugal equalled the centripetal force, and therefore equatorial rings were necessarily abandoned to form satellites. By calculation I found that in the cases of Mars, Venus, and Mercury their nebular expansions were not far enough, and their force of gravity was not strong enough, to produce a rotation that could abandon equatorial rings. Our earth, from want of mass and want of nebular expansion, came very near to having no satellite, no friendly, softly shining moon.

Having compared the power of gravity as the only projectile force with the actual velocities of the planets, asteroids, and satellites, we may advance an important step and prove the general abstract proposition, that, in all greatly expanded nebulæ, after rotation has once begun, and during a slow contraction, the force of gravity is sufficient to cause a velocity such that the centrifugal must equal the centripetal force—in other words, that the falls toward their centres from infinite space will be more rapid than the velocities required in their planets when formed. This demonstration is aided by the following diagram (fig. 5). The notation is the same as that in the formula $V = \sqrt{2gr}$, already given.   Fig. 5.

Let A B E be the orbit of a planet. E B an arc passed through by a planet in 1 second. Then E D = B F = distance fallen toward the sun in 1 second = $\frac{1}{2} g$. A E = twice the distance of a planet = $2r$. B E (very nearly arc B E) = the velocity of planet in orbit = v.
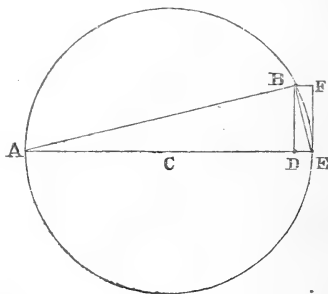
E A : B E :: B E : D E,

$2r$: v :: v : $\frac{1}{2}g$; $\therefore v = \sqrt{gr}.$

But the velocity acquired by a fall from infinite distance = $2gr$; $\therefore$ planet's velocity in orbit : velocity of fall from infinite distance :: $1 : \sqrt{2}$.

We may now pass on to the demonstration that in a very large nebula, whose rotation pervades the interior throughout as well as the exterior, the velocity of rotation may become sufficient to abandon all its material by centrifugal force,

leaving a system of stars with no central sun. We may suppose, for instance, our solar nebula to have been expanded originally to less than half the distance to the nearest fixed star, say to 3500 times the distance of Neptune. Then much less than the 40,000,000,000th of the entire mass would have been within the present orbit of Neptune. All the matter exterior to that orbit would have been abandoned as equatorial rings; and even the matter at that orbit, and some within, would have been so abandoned. This appears as follows. We have just seen that the velocity of a planet in its orbit is to a fall from infinite distance as 1 is to the square root of 2. We take the velocity of Mercury in its orbit as 110,000 miles per hour, and that of Neptune as 12,500 miles per hour; therefore

$$1 : \sqrt{2} :: 110,000 : 154,000$$
$$1 : \sqrt{2} :: 12,500 : 17,625$$
$$\text{Difference} \quad . \quad \overline{136,375}$$

Therefore a fall from the orbit of Neptune to that of Mercury would gain a velocity of 136,375 miles per hour, which is far more than that of Mercury. Hence a nebula rotating through all its interior, and contracting through a distance from Neptune to Mercury, would gain a velocity great enough to separate by centrifugal force nearly all of its interior matter; and therefore all its matter exterior to that orbit would be separated, leaving an excessively small amount to subside at the centre. With so small an amount of matter within that orbit, the force of gravity toward the centre would be very small, and the consequent velocity of rotation and centrifugal force very small. But the *proportion* between the centrifugal and centripetal forces would remain the same, and the equipoise between the two would occur at the same distance from the centre, as if they were both large.

Corollary 1.—An ancient nebula vastly expanded, whose primitive rotation pervaded its entire mass, might gain a centrifugal force to abandon nearly all its materials as rings far away from its centre; and as contraction continued, those rings would break and condense into stars forming an annular system like that in Lyra, or like our own sidereal system, whose stars are chiefly in the galactic ring or rings.

Cor. 2.—A nebula with a smaller amount of rotation in its interior at the beginning, would not abandon its chief materials so far from the centre. But it might abandon its materials evenly from the circumference to the centre, and thus form a sidereal system which would appear in the vast distance like what are called planetary nebulæ.

Cor. 3.—A nebula with a still smaller amount of primitive rotation through its interior would abandon a still smaller amount of its materials toward its circumference, and a larger amount in the neighbourhood of its centre, and would form a sidereal system with very many stars located closely together near the centre. When seen by us obliquely it would be called an elliptical nebula.

Cor. 4.—A nebula with only a shallow surface-rotation, and no rotation through its vast interior, in the beginning, might collect the most of its materials in one large body at the centre. Its abandoned rings might indeed form millions of stars; but from a vast distance they would appear as a nebulous haze around the great central body. Such a sidereal system we should call a nebulous star.

Scholium.—All sidereal systems formed by the action of gravity on slowly contracting nebulous masses must not necessarily have regular forms, either round or elliptical. Thus millions of individual suns may revolve around their common centre of gravity, but be irregularly situated—just as our own solar system must appear from a distance as a very irregular cluster.

Cor. 5.—We know of but two forces at work in forming the sidereal, the solar, and the planetary systems—the force causing the condensations, and the force causing the rotations. The condensations were caused by the chemical force, the same as when oxygen and hydrogen combine and condense into water. The nebular condensations could not have been caused by the radiation of heat; for when all matter was expanded through all space the heat could not radiate away, for there was no other space where it could go. Elsewhere I have endeavoured to prove that the ancient nebulæ were great chemical laboratories, evolving those modifications of matter which we call simple elements, and evolving now the light and heat of the sun and the other stars.

Cor. 6.—The centrifugal force in astronomy arose from the conversion and conservation of the force of gravity. Faraday, with all the simplicity and directness of great genius, says that "inertia is always a pure case of the conservation of force." Among the stars the centrifugal force, or inertia, sprang from the rapid revolutions; and the rapid revolutions sprang from gravity. Therefore the centrifugal and the centripetal forces, whose actions are in opposite directions and antagonistic, have the same origin. Among all the cases of the conversion of the physical forces, this is the only one where we can understand how it is done.

Scholia.—The nebular theory, as here expounded, is a very

different thing from the nebular theory of Laplace. Laplace did not attempt to account for the origin of the sun, of the stars, of the comets, or of the meteors. Neither did he assign any cause for the solar rotation. After assuming that the atmosphere of the sun had been suddenly expanded by some extraordinary and unknown cause, he did not prove that, on contracting again, the velocity of rotation could become so rapid as to make the centrifugal equal to the centripetal force, and thus abandon rings to form planets and satellites. Hence, in giving his theory to the world, he says, "This origin of the arrangements of the planetary system I offer with all that distrust which every thing ought to inspire which is not the result of observation or *calculation.*" Neither did he write any thing which indicated a surmise that gravity was the force which in the beginning imparted all their velocities to all the stars.

Hitherto it has been claimed for no one, except for Compte, that he had calculated the velocity of nebular rotation. But really he came far from doing this. He simply *assumed* that at the planetary orbits the centrifugal and the centripetal forces in the solar nebula became equal; and then he computed what must have been the velocity of nebular rotation to produce this equipoise. He found, on that *assumption,* that the equatorial velocity must have coincided with the present planetary velocities, excepting only one forty-fifth part of the present velocities of the planets. But this is solving a very different problem from what is demanded. We demand to know how and why the nebular and planetary velocities became what they are, how and why the centrifugal and centripetal forces became equal. This I have answered. Moreover he could not understand the reason of the discrepancy in his calculations between the nebular and planetary velocities—one forty-fifth part. This I found to arise from the wrong determination of the earth's distance from the sun, 95 millions of miles instead of 92 millions.

Philadelphia, Feb. 5, 1877.

XXXVI. *Notes on the Theory of Sound.* *By* R. H. M. Bo-sanquet, *Fellow of St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN a revision of this subject recently undertaken with a view to the preparation of an elementary treatise, I have come across a few points which seem of sufficient interest to be

placed before the readers of the 'Philosophical Magazine.'
These I propose to submit to you.

<div align="center">Yours &c.,

R. H. M. Bosanquet.</div>

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1. *The Velocity of Sound, and Ratio of Specific Heats, in Air.*

On examining the received values of the velocity of sound,
it becomes apparent that the discordance between the results
of different experimenters is considerable.   I propose to
put the results together and compare the different experi-
mental data which can be brought to bear on the subject.
Without attempting to give a final result, I think some con-
siderations of interest can be suggested as to the values that
most nearly satisfy the different determinations and require-
ments.

The following Table represents the velocities of sound found
by different observers, reaching from the middle of the last
century up to the present time :—

Names of observers.	Velocity of sound. metres.
Académie des Sciences (1738)......	332
Benzenberg (1811)	{ 333·7 / 332·3
Goldingham (1821)	331·1
Bureau des Longitudes (1822)......	330·6
Moll and Van Beek	332·2
Stampfer and Myrbach...............	332·4
Bravais and Martins (1844).........	332·4
Wertheim	331·6
Stone	332·4
Le Roux.....'.........................	330·66
Regnault..............................	330·7

It is clear that we cannot come to a final conclusion on these
premises as to the number to be adopted.   I do not think
it is a case where the method of least squares is applicable ;
for the differences appear to be systematic ; and the question
is, which observers do we really trust most?

The difficulty of the application of the method of discharges
of cannon to the determination of an element, in the theory of
which all motion is supposed continuous and all displacements
small, has been frequently noticed.   The exhaustive researches
of Regnault[*] on the propagation of disturbances in tubes, in
which they were examined at different periods of their course,

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[*] *Mém. de l'Institut*, 1868.

throw a light on the subject which appears to have been insufficiently appreciated.

In the next place, his proceedings bring to light the difficulty in correctly allowing for wind &c. He always employed reciprocal discharges in the open-air experiments ; but it was clear that accidental influences were not entirely eliminated. This consideration alone furnishes an objection fatal to the authority of all experiments not conducted by reciprocal discharges.

There is also a question as to the direction of transmission not being rectilinear in the neigbourhood of the gun, the error arising from which Regnault's arrangements eliminated.

The memoir occupies the earlier half of the volume of memoirs of the Institute for 1868 ; I cannot undertake to reproduce even a sketch of the results, more especially as a short summary by Regnault himself was printed at the end of the former edition of Tyndall on Sound.

The result of the study of propagation in tubes above alluded to is, that the disturbances arising from the class of sounds employed (pistol-shots) had for some distance from their origin a velocity comparable with the value ordinarily received for the velocity of sound, that as the disturbance diminished its velocity grew less ; and the last velocity registered by the apparatus in tubes, even 1 metre or more in diameter, was always less than the value 330·7, which he announces as his *mean* result for the open air.

The *mean* results always denote the mean velocity from end to end of the course. There is much that is difficult in the reception of these *mean* velocities, which are really fictitious quantities, for any theoretical purpose. But the mere recognition of their nature, and the approximate distinction drawn between them and the true velocities at different parts of the course, constitute really a greater advance in the subject on the part of Regnault than any thing that we have received from other experimenters. The influence of the diameters of the tubes containing the gases, too, is an element of the utmost importance. For instance, in the Table just given there is a determination by Le Roux[*] which appears to coincide closely with Regnault's ; but on consideration it is seen that in all probability two different elements enter into the determination, either of which would alone produce a deviation, and an accidental compensation occurs. The disturbance is taken in the earlier stage, when, according to Regnault, the velocity would be considerably greater ; and it is propa-

[*] *Ann. de Chim. et de Phys.* 4e sér. vol. xii. p. 345.

gated backward and forward through a tube ·07 metre in diameter, which should diminish the velocity.

As between Regnault's results and those of any one else, I have no doubt that in Regnault's we know far more about the different elements of the question in a trustworthy manner than in any other case. But it does not follow that his number is to be taken as true to the first place of decimals.

If we examine the isolated determinations, we find that the deviations are not inconsiderable: in some cases they amount to fully the difference between Regnault's own number and 332 metres, or thereabouts, which is more usually accepted.

Under these circumstances we can form no definite conclusion from the numbers before us; but I think that if we take Regnault's number (330·7 metres) and the ordinary number (332 metres), the mean between them, 331·35 metres, or thereabouts, will afford a number which will be safe to work with, and may be used with a certain reserve as representing the results of experiment.

There is a point in the kinetic theory of gases upon which this number has a bearing of great interest, as every body knows. It may be that the particular detail I am about to mention is well known; but I have never come across any allusion to it.

The connexion between the ratio of the specific heats and the observed velocities of sound is as follows (I insert the velocities in feet here, as we are generally here accustomed to reckon this particular datum in this way):—

Ratio.	Velocity of sound.	
	feet.	metres.
1·41	1090·7	332·4
1·40	1086·8	331·2
1·395	1085·0	330·6

These numbers are reckoned with data which give 918·5 feet for the Newtonian velocity.

Regnault, selecting the velocity which he applies to the specific heats, and applying small corrections, gets 1·3945 for the ratio he adopts.

It has been long recognized that a relation exists between the nature of the particles of a gas and its ratio of its specific heats. The first essential law is that first proved, I believe, by Clausius [*], that the ratio of the *vis viva* of translation to the total *vis viva* in a gas is $\frac{3}{2}\left(\frac{c'}{c}-1\right)$, where $\frac{c'}{c}$ is the ratio of specific heats. And the second is that given by Max-

[*] Pogg. *Ann.* 1857, vol. c. p. 353 (*Abhandlungen* &c. 2te Abth. p. 258).

well *:—The average *vis viva* along each of the three axes is the same, and = average *vis viva* of rotation about each of the three axes.

If we applied these laws to the old value of ratio of specific heats (1·41), we got ·615 as the ratio of *vis viva* of translation : total *vis viva* ; and this was unintelligible.

Kundt and Warburg †, starting from certain chemical considerations as to the molecular constitution of mercury, were led to investigate the ratio of specific heats in its vapour by measuring the length of stationary sound-waves produced in it. They found the ratio = 1·186 × that in air, whence, assuming air = 1·405, they got 1·666 for the ratio in mercury. (If we put ratio in air = 1·4, we get 1·660.)

Their reasoning is then somewhat as follows :—If we regard the atom of mercury as a perfectly smooth hard sphere, the specific-heat-ratio of a gas composed of such atoms should be $1\frac{2}{3}$, or very nearly the value found.

This well-known conclusion follows from the above principles by remarking that the *vis viva* of rotation is absent in the supposed case, and consequently the ratio, *vis viva* of translation : total *vis viva* is unity ; whence, from Clausius's equation,

$$\frac{c'}{c} = 1\frac{2}{3}.$$

The objection to the application of this reasoning to any real gas such as mercury is that the spectroscope tells us that vibrations go on inside the molecule (or atom), and in that case the total *vis viva* would include that due to these vibrations.

All that can be said, then, is that the theory cannot at present take count of these intramolecular (intraatomic) vibrations. We have no theory about any of these matters that is quite perfect ; but the gradual advance has consisted, and is likely to consist, in the recognition of general correspondences, as in the case of the dynamical theory of optics. From this point of view we may admit the result of Kundt and Warburg as one of interest and importance, while fully keeping in view the incompleteness of the dynamical explanation it affords.

Now it is but a step from the case of a smooth hard sphere to the case of a smooth hard solid of revolution, which we may conceive of as formed of two smooth hard spheres rigidly joined up together, or as a cylinder, or having many other forms.

A gas constituted of such particles would have five equal portions of *vis viva*, three of which would be due to translation,

* Phil. Mag. [IV.] vol. xx. p. 36.
† Pogg. *Ann.* vol. clvii. p. 353.

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two to rotation; for by the hypothesis that the body is a solid of revolution, perfectly hard and smooth, *vis viva* of rotation about the axis of revolution is excluded.

The Clausius equation then takes the form

$$\tfrac{3}{5} = \tfrac{3}{2}\left(\frac{c'}{c} - 1\right),$$

which gives

$$\frac{c'}{c} = 1\tfrac{2}{5}, \text{ or } 1\cdot40.$$

The ratio of the specific heats for air, as deduced from the value of sound, cannot, as we have seen, be assigned with great accuracy from a comparison of experimental data; all we can say is that the value of the velocity of sound, deduced from ratio 1·40, differs from our mean value by an amount insignificant compared with the difference of the mean value from the results of the various experimenters. (Mean value = 331·35 metres, value due to 1·40 = 331·2 metres.)

If, then, Kundt and Warburg's result is worth any thing at all, it is clear that the explanation of the ratio of specific heats in air and allied gases may be placed on a perfectly analogous footing.

The relation is so obvious that it is impossible to suppose that it has not occurred before to the eminent men who have dealt with the subject. I will notice the points of objection that occur to me; there may be others of a more conclusive character.

It has been frequently assumed that if two or more atoms are built up into a molecule, they must be connected in such a manner that they can oscillate with respect to their common centre of gravity. Every degree of freedom in such oscillations should, it appears, retain the same *vis viva* as a degree of freedom of the motion of the molecule itself[*]. Under these circumstances, if we supposed two spherical atoms joined by elastic forces, the system would have more than five degrees of freedom.

If we ask, why must we suppose the two atoms joined by elastic forces and not rigidly? we are told that it is the vibrations of the atoms that do the work we see in the spectroscope lines. But these lines occur in the vapour of mercury, for which our explanation fails to suggest any collocation of atoms more than one in the molecule. Why not, then, admit that the lines are produced by something within the atom which we

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[*] Boltzmann, *Ber. d. Wien. Akad.* vol. lxiii.

cannot at present account for, just as we cannot decompose it chemically *?

I suppose, however, that the principal objection would have been that the experimental data were not consistent with the hypothesis. In answer it seems to be enough to look at the Table at the beginning of this note; we can say without hesitation that, giving due weight to Regnault's experiments, it is impossible to conclude with any certainty whether the true value of the ratio of specific heats lies above or below 1·40.

At the same time, since there must be internal work done in the atoms in some way that we do not comprehend, though it is probably very small, we may regard such a value as Regnault's (1·3945) as compatible with a possible explanation, since with such a value the total *vis viva* is slightly greater than that due to the 5 degrees of freedom.

The admission of the number 1·40 as a working constant involves relations between the mechanical equivalent of heat and the specific heat of air at constant pressure.

The following numbers indicate the nature of the relation, the ratio of specific heats of air being taken at 1·40:—

Mechanical equivalent.	Specific heat at constant pressure.
431	·2377 (Regnault)†
429	·2389 (Wiedemann)
424 (Joule)	·2416

Regnault's own computation of the mechanical equivalent gives 436·1.

A comment of Professor Foster on recent determinations in the Supplement to Watts's 'Dictionary of Chemistry (1872, p. 687), after noticing the general accordance of modern results in values higher than Joule's original number (424), proceeds:—" Joule's new result (429·3) is the lowest of three values obtained in distinct sets of observations; but it is adopted by him as the result of the investigation, in consequence of more complete precautions to ensure accuracy having been taken in the set of experiments from which it was deduced

* There is nothing to prevent us from forming the conception of very small amounts of energy existing in some way or other within an atom (regarding this as already complex in a manner which we do not understand), though we are unable to give any accurate account of the way in which the movement is originated or maintained.

† Regnault's determinations varied from ·23536 to ·23890, Wiedemann's from ·2374 to ·2414 (Pogg. *Ann.* vol. clvii. p. 21). This last limit comes very near the number required by Joule's value of the equivalent.

than in the other two.   These, however, were closely accordant
with each other, and would lead to 431·5 as the value of the
mechanical equivalent."

In view of this comment, the above values cannot be
regarded as beyond admissible limits.   We may conclude,
then, that the values 1·40 for ratio of specific heats, and 331·2
metres or 1087 feet for the velocity of sound, are a fair repre-
sentation of experimental results—and that they give rise to
no discordances in their connexion with other data and theories,
which are of an amount to call for rectification on the evidence
at present before us.

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P.S.—Since the above was written, and after my forth-
coming little work, in which the matter is similarly treated,
has gone to the printers, I read in the number of Poggendorff
just received (1877, No. 1, p. 175) a short note from Boltz-
mann, being an abstract from a paper in the Vienna *Berichte*,
in which he shortly announces the above result and his adhe-
sion to it.   It is stated that the experimental values of sound
are discussed in the original paper; so that the substance of
my communication is anticipated.   I have, however, thought
that it may still not be entirely without interest.

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XXXVII. *A Theory of the Action of the Cup-shaped Radio-
meter with both sides bright.   By* Professor CHALLIS, *M.A.,
F.R.S., F.R.A.S.*[*]

OUT of the many experiments relating to the radiometers
described by Mr. Crookes in his communication con-
tained in No. 175 of the 'Proceedings of the Royal Society,'
I have selected for theoretical consideration No. 1035, in
p. 313, because this experiment exhibits a novel and signifi-
cant phase of the action of the instrument.   It is evident that
in this instance the radiometer is caused to rotate simply be-
cause the vanes are convex on one side and concave on the
other, the conditions as to the incidence of light or heat being
exactly the same for both; and accordingly a theory of the
motion must explain how the effect is produced solely by the
difference of the two sides as to *form*.   Such an explanation
will, I think, be found to be given by the following theory,
which rests on the same principles as those applied in the ex-
planation of the phenomena of the radiometer in two prece-

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[*] Communicated by the Author.

ding communications contained in the Numbers of the 'Philosophical Magazine' for May and November 1876, and at the same time departs in no respect from the principles of the general hydrodynamical theory of the physical forces which I have now for a long time upheld.

Considering, first, the case of the radiometer with the vanes blackened on one side, I assume that the radiant light or heat which is incident on the vanes, being thereby converted into heat of temperature, causes the atoms in a thin superficial stratum to be displaced from their neutral positions, and in greater degree on the blackened side than on the other, on account of the greater accession of temperature on that side. It is true that the intrinsic molecular forces of the vanes will tend continually to make the atoms return to their neutral positions; but since at the same time the disturbing force is continually in action, the result of the antagonistic forces, so long as the disturbance is operative, will be a persistent abnormal condition of the superficial stratum. This is a *real* change of condition of the vanes, by whatever name it be called. It seems to me that it would be best described by the term *thermo-electric*. But it is of chief importance to remark, that every such superficial disturbance, however caused, gives rise to *a steady circulating ætherial current,* in which the pressure varies so as to be always less the greater the velocity, and that consequently atoms immersed in such a current will be dynamically acted upon by reason of the variation of pressure. In the present instance the course of the current is from the blackened to the opposite surface, because, on account of the greater expansion of the vane on the warmer side, the channel for the current diminishes and the velocity increases towards the bright side, and accordingly the dynamical action is the same as if the vane were *pushed* on the blackened side. [I have so fully discussed the above-cited hydrodynamical proposition in previous communications, that I do not think I am called upon to produce the demonstration here.]

Taking now the case of the cup-shaped radiometer (No. 1035), we may assume that when the disturbing effect of the incident radiant light or heat, converted into heat of temperature, is just co·nterbalanced by the tendency of the superficial atoms to take the positions in which they are ordinarily held by the molecular forces, they will, as in the former case, be permanently displaced from their neutral positions. But because both the radiating surfaces are bright, the action will be the same on both, so far as regards the amount of expansion caused by the accession of temperature. There will, however, be the differ-

ence, that a given displacement in the normal direction will cause the atoms in a *given* superficial area to be spread over a *larger* portion of convex surface than of concave surface, the motion in both cases being from within to without, and the lines of motion being divergent for the convex surface and convergent for the concave surface. On this account there will be, *cæteris paribus*, greater atomic density at the concave surface than at the convex surface, and the latter will be in the condition of the blackened surface of the ordinary radiometer. Hence the cup-shaped radiometer will move as if it were pushed on the convex side, as is found experimentally to be the case.

The experiments referred to showed that the motion of the radiometer was in the same direction and nearly the same in amount, whether the light fell only on the convex side, or only on the concave side, and that in either case the rate of revolution was about half what it is when the light falls on both sides. These results may be considered to be consistent with the theory, inasmuch as we may draw the inference from the supposed *thermo-electrical* character of the disturbance, that the state of displacement of the atoms induced by a disturbance on one surface is spread equally over both by *superficial conduction*, and consequently that the action is of the same kind whether one surface or both be illumined.

When this radiometer was heated by a hot shade or plunged in hot water, rotation was caused in the opposite direction to that caused by the light. Under either of these circumstances the superficial atomic density of a vane must be the same throughout in order that its molecular forces may counteract the effect upon it of the *uniform* surrounding temperature. But to satisfy this condition at the convex surface, not only must the tendency of the induced heat to diminish the superficial atomic density by expansion be counteracted, but that also which is due to the *divergence* of the lines of motion normal to the surface ; whereas at the concave surface the *convergence* of the lines of motion *helps* to counteract the tendency of the temperature to diminish the superficial atomic density. Hence the molecular force *from without to within* acts in *greater* degree at the convex surface than at the concave surface. Hence also the interior atomic density diminishes in the direction from the convex to the concave surface, because where the molecular force is greater, the atomic density must, *cæteris paribus*, be greater. Consequently, as the greater atomic density is on the convex side, the greater velocity of current and less density of the æther is also on that side, so that the vane is urged in the direction from the con-

cave to the convex side. Thus it appears that the motion of
the radiometer due to the hot shade or hot water is opposed in
direction to that caused by the light. This result accords
with the experiment mentioned at the top of p. 314 of the
' Proceedings.'

The most essential part of the theory proposed above is the
dynamical action of a circulating ætherial current. From
hydrodynamics we know that such a current could not *begin* to
flow in a perfect vacuum, and that it flows with more difficulty
in proportion as the vacuum is more complete. This is exem-
plified by the difficulty of producing a galvanic discharge in
very rare air between distant poles. The proposed theory,
therefore, gives a sufficient reason for the decrement of the
action of the radiometer as the exhaustion of the air advances
towards completion. From this point of view I am wholly
unable to admit that the presence of residual gas is the *cause*
of the movement of the radiometer. It seems to me that one
might as well argue that because a bell is not heard when rung
in an exhausted receiver, the air in the receiver, and not the
tongue of the bell, produces the sound when it is heard.

I take occasion to say here, as I have said before, that I
have been induced to give particular attention to theoretical
explanations of phenomena of the radiometer solely on account
of the adaptability of such explanations to elucidate the hy-
drodynamical theory of the physical forces.

Cambridge, March 13, 1877.

XXXVIII. *Observations on Gallium.* By M. M. PATTISON
MUIR, *F.R.S.E., Assistant-Lecturer on Chemistry, the Owens
College, Manchester.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your Magazine for 1876 you published a translation, by
me, of a short paper written by M. Mendelejeff which ori-
ginally appeared in vol. lxxxi. of the *Comptes Rendus,* p. 969.
In this paper Mendelejeff drew attention to the "periodic law"
announced by him in 1869, according to which "the proper-
ties of the elementary bodies, as also the properties and con-
stitution of their compounds, are periodic functions of the ato-
mic weights of the elements." In accordance with this hypo-
thesis, the elements may be arranged in certain groups;
remarkable relations are shown to exist between the proper-
ties of the members of each group. The third group com-

prises certain of those elements which form oxides having the
general formula $R_2 O_3$, viz. *boron*, having the atomic weight
of 11, *aluminium* $= 27$, *yttrium* $= 88$, *indium* $= 113$, *didy-
mium* $= 138\,(?)$, *erbium* $= 178\,(?)$, and *thallium* $= 204$.   But
when this group is compared with others with which it ought
(theoretically) to present certain analogies, it becomes evident
that there are several gaps in the series.   These gaps M. Men-
delejeff has filled with hypothetical elements.   Two vacant
places occur between aluminium and indium ; to the second of
these the hypothetical metal ekaaluminium is assigned.   The
analogies between this metal and aluminium, on the one hand,
and indium, on the other, should be, according to theory,
somewhat the same as those existing between zinc and magne-
sium and cadmium, or arsenic and phosphorus and antimony.

In the paper already referred to, the theoretical properties of
ekaaluminium and of some of its salts are detailed.   M. Men-
delejeff then puts forward the suggestion that the new metal
gallium, discovered August 3, 1875, by M. de Boisbaudran, is
very probably no other than the hypothetical ekaaluminium.

Since the publication of Mendelejeff's paper, various notes on
gallium and on its salts have been published by the discoverer.
In many respects the observed properties of this metal corre-
spond with the properties which Mendelejeff's theory assigns to
ekaaluminium.   I have tabulated the leading properties of
gallium and of its salts, and have placed alongside of these
descriptions others, of the properties of the analogous salts of
aluminium and indium.   I have also tabulated a few of the
hypothetical properties of ekaaluminium as detailed by Men-
delejeff in the paper already referred to.

M. de Boisbaudran does not apparently altogether accept
the position assigned to ekaaluminium for his new metal gal-
lium.   Further researches must of course be forthcoming
before a decided opinion can be given.   Would it not be well
that these researches should in the first place take the direc-
tion indicated by the hypothetical properties of ekaalumi-
nium ?   Mendelejeff's hypothesis is at least of much value as
a guide to future research.

                    I am, Gentlemen,
                              Yours truly,
                                    M. M. PATTISON MUIR.

The Owens College, Manchester.
        February 24, 1877.

1. Aluminium.	2. Gallium.	3. Indium.	4. Hypothetical Ekaaluminium.
White, very lustrous, and very ductile. Melting-point over 400°. Not oxidized in air, even at highest temperatures, when in divided state, burns easily in oxygen. Specific gravity 2·67. Not attacked by nitric, but readily soluble in hot nitric acid. Soluble in potash or soda with evolution of hydrogen.	Nearly white; when melted, remains as lustrous white liquid at temperatures below the melting-point of the solid metal. Somewhat malleable: may be cut with a knife. Melting-point = 30°·15. Heated to redness in air, non-volatilized, and but superficially oxidized. Crystallizes in octahedra. Specific gravity 5·956. Hardly acted on by nitric acid diluted with its own volume of water. Dissolved by hot nitric acid. Precipitated from alkaline solutions by electric current.	Silver-white, ductile, very lustrous. Melting-point = 176°. Unchanged in air at ordinary temperature; burns when strongly heated in air. Specific gravity = 7·3–7·4. Insoluble in nitric acid: dissolved by dilute hydrochloric or sulphuric acid.	Should be readily obtained by reduction. Nearly non-volatile. Melting-point tolerably low. Specific gravity = 5·9. Not acted on by air; will decompose water at a red heat. Slowly attacked by acids or alkalies.
Precipitated by sulphuretted hydrogen from neutral or alkaline solutions.	Precipitated by H_2S before zinc. If solution of sulphides of gallium and zinc in hydrochloric acid be treated with sodium carbonate, first portions of precipitate contain most of gallium; cannot, however, be thus separated from aluminium: indium comes down only after gallium completely precipitated. Precipitable by barium carbonate.	Precipitated from neutral solutions, or from solutions containing acetic acid by sulphuretted hydrogen. Solutions precipitated by sodium carbonate (see column 2). Also by ammonia; latter precipitate insoluble in excess, but dissolved by caustic potash or soda.	Hydrate amorphous, insoluble in water, but soluble in acids or alkalies. Solutions should yield precipitate with barium carbonate. Will form a potash alum more soluble, but less easily crystallizable, than the corresponding aluminium salt. Formula of oxide, Ea_2O_3; chloride, Ea_2Cl_6.
Solutions precipitated by sodium carbonate (see column 2).			
Chloride deliquescent; solution yields precipitate (hydrate) with ammonia, insoluble in excess, but soluble in caustic potash or soda.	Gallium chloride deliquescent and easily soluble: solution becomes turbid on dilution with water; precipitate, probably an oxychloride, is with difficulty soluble in hydrochloric acid.	Chloride very deliquescent; forms oxychloride by heating; soluble in water.	I do not add the remaining properties of ekaaluminium, because the researches of De Boisbaudran upon gallium do not allow us to say whether these properties are characteristic of the new metal. If one were to tabulate the properties of zinc, magnesium, and cadmium, as also those of arsenic, phosphorus, and antimony, I think the general analogies of each of these groups with the group aluminium, gallium, indium would, even now, become apparent. Further, I think that the hypothetical properties of Mendelejeff's ekaaluminium show a remarkable coincidence with those of gallium, so far as the latter have been examined.
Sulphate easily soluble in water, and yields an alum with ammonia or potash.	Gallium sulphate non-deliquescent, soluble in water. On addition of ammonia and evaporation an alum is formed.	Sulphate non-crystalline. Solution in water apparently forms double salts (alums) with potassium sulphate.	
	Solutions of chloride and sulphate yield precipitates (hydrates) with ammonia which are partially soluble in excess of the precipitant.		

XXXIX. *Short Reports from the Chemical Laboratory of Trinity College, Dublin* (Nos. 2 *and* 3). *By* J. Emerson-Reynolds, *M.D., M.R.I.A., Professor of Chemistry, University of Dublin*.*

No. 2.—*On Franklandite, a new Mineral Borate.*

MANY analyses have been made from time to time of sodio-calcic borates, brought chiefly from the Peruvian province of Tarapaca. Most of the analyses seem to show that the specimens of borates examined were mixtures of some predominant mineral, such as the well-defined Ulexite, with varying quantities of one or more borates containing a larger proportion of sodium than Ulexite. I have lately had the opportunity of examining a specimen of a borate from Tarapaca which has the same components as Ulexite, namely sodium, calcium, boron, oxygen, and water, but which is much richer in alkali than that mineral, and which seems to be a nearly pure specimen of the mineral with which I supposed Ulexite to be often associated. The specimen I analyzed was brought from Peru by Mr. Graves, and was kindly placed in my hands for examination by the Rev. Dr. Haughton.

The specimen consists of a felted mass of long, thin, interlaced fibres, which are white in colour and possess a silky lustre. Under the microscope, the fibrous crystals taken from the cleanest portions of the mass were seen to be free from any but traces of granular coating, and presented the same general aspect.

The hardness is not greater than 1 ; and the specific gravity proved to be 1·65.

The mineral is easily fused, first losing much water.

The taste of the mineral is first slightly saline, owing to the presence of a little admixed common salt, and is afterwards somewhat alkaline. It is slightly soluble in water, but is easily and almost completely dissolved by dilute hydrochloric and nitric acids, the minute residue consisting of particles of clay.

When portions of the mineral mass were digested with nearly ice-cold water, a sensible quantity of chloride was dissolved out. The solution, evaporated on a glass microscopic slide, afforded the well-known crystals of sodic chloride. A little calcium was also dissolved by the water. In some of my earlier experiments upon this body I was led, through the carelessness of a junior assistant, to suppose that the percentage of chloride present was much greater than it is

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* Communicated by the Author, having been read before the Royal Irish Academy.

shown to be in the analysis given further on. The specimen taken for this analysis was carefully picked by myself from the best parts of the mass and the selected portions then mixed.

The analytical process was conducted in the following way :—

A weighed quantity of the borate was dissolved in pure dilute nitric acid ; to the solution excess of silver nitrate was added, and the precipitated chloride of silver filtered off and determined. The excess of silver was then separated from the filtrate by just sufficient hydrochloric acid ; a calculated excess of pure oxalic acid was next added, and the solution then rendered alkaline by ammonia. The calcic oxalate thus separated was determined in the usual way. The filtrate from the oxalate precipitate was then acidulated with hydrochloric acid, and a small quantity of sulphuric acid separated and estimated with the aid of baric nitrate, the excess of the latter being in turn removed by treatment with sulphuric acid. The solution was now transferred to a platinum dish and evaporated to a small bulk, some pure oil of vitriol added, and the heating continued in order to destroy any oxalic acid present. After evolution of gas had ceased, the liquid was allowed to cool and then treated with pure concentrated hydrofluoric acid, and heat applied to volatilize the boric fluoride. The hydrofluoric-acid treatment was twice repeated, and the heating then continued until the temperature ultimately rose to redness. The residue, consisting of sodic and potassic sulphates, was weighed, the potassium present determined with platinic chloride, and from the data obtained the proportion of potassic and of sodic sulphates then calculated.

The total water was determined by ignition of a weighed quantity of the mineral in a combustion-tube with pure dry litharge ; the water was collected in a calcic-chloride-tube and weighed, the whole operation being conducted as in the ordinary process of organic analysis.

In order to determine by difference the percentage of boric anhydride in the mineral, the small quantity of sulphate present was assumed to be in the form of calcic sulphate, while the chlorine found was supposed to be combined with the whole of the potassium and with its equivalent of sodium. The residual sodium and calcium were then calculated into their oxides, in accordance with the general practice of mineralogists. The sum of the percentages of impurities, sodic oxide, calcic oxide, and water, subtracted from the hundred parts, then gave the percentage of B_2O_3 by difference. This result was controlled by a direct determination as potassic boro-fluoride, made in the following way. The borate was dissolved in the least possible quantity of dilute hydrochloric acid ; a slight excess of

pure oil of vitriol was then added, and the liquid diluted with twenty volumes of strong alcohol. Calcic sulphate and most of the sodic sulphate separated out; the filtrate when evaporated, after addition of an excess of pure potassic hydrate, was treated with pure hydrofluoric acid, the mixture evaporated to dryness, and the insoluble salts washed away from the resulting potassic boro-fluoride by solution of potassic acetate (1 to 4 of water) followed by alcohol. Estimation of boric oxide by means of the boro-fluoride is always unsatisfactory, owing to the solubility of the body, and to the large volume of potassic acetate solution required for the removal of the saline impurities; nevertheless it is useful as a check on the indirect determination of the boron compound. The numerical value obtained by the latter method is much more worthy of confidence, though probably slightly in excess of the true amount.

	Theory.	Experiment.		
		I.	II.	III.
B_2O_3	43·61	43·76 (by diff.)	41·81 (direct)	
CaO	11·63	12·10	...	11·94
Na_2O	12·87	12·37		
H_2O	28·04	27·92	...	27·66
(Na, K)Cl = 2·41 ...... $CaSO_4 + 2aq. = 1·44$ ...	...	3·85		
		96·15	100·00	

If we exclude the impurities present and deal with the residue only (96·15), the analytical data (I.), when discussed in the usual way, lead well to the empirical formula which may be stated thus, according to the custom amongst mineralogists :—

$$2\,Na_2O, 2\,CaO, 6\,B_2O_3, 15\,H_2O.$$
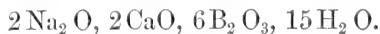
Our insight into the constitution of compound borates is so limited that it is scarcely safe to assign to a new member of the class, as I believe this body to be, a " rational " formula ; nevertheless it is possible to express the composition of the compound in such a way as to exhibit very clearly its probable relationship to Ulexite ; for the two minerals may be thus represented :—

Ulexite,
$$[2(Na'BO_2) + 2\,H_3BO_3] + 2[Ca''(BO_2)_2 + 2\,H_3BO_3] + 9\,H_2O.$$

Franklandite,
$$[2Na'BO_2 + 2\,Na'\,BO_2] + 2[Ca''(BO_2)_2 + 2\,H_3BO_3] + 9\,H_2O.$$
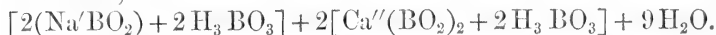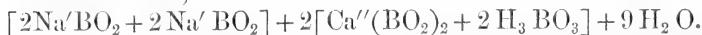
I have ventured to assign to the new mineral the name

" Franklandite," with the permission of the distinguished chemist whose researches on the organo-boron compounds have contributed so materially to extend our knowledge of those interesting bodies.

Although the formulæ given above for the two minerals are not in any true sense " constitutional," they make one point clear, namely that the substitution of one molecule of sodic oxide (Na_2O) for three molecules of water is capable of converting Ulexite into Franklandite, so far at least as composition is concerned. As a change of this kind can evidently be effected by various indirect means, it is to be expected that mixtures of the two borates in different proportions would be found ; and the observed differences in the analytical results afforded by some specimens of native sodio-calcic borates seem to indicate that such mixtures are not uncommonly met with.

No. 3.—*On the Composition of Lievrite, as determined by* Mr. Early's *method*.

Of the several methods which have been devised for the analysis of ferroso-ferric silicates, that which has been published by Mr. William Early [*], Demonstrator of Chemistry in this Laboratory, is probably the most easily managed. The advantages attending its use are chiefly felt in analyzing silicates, which are either insoluble in, or attacked with difficulty by the ordinary acids ; but it can also be used with great convenience in the analysis of silicates easily acted upon by acids. Lievrite is a silicate belonging to the latter class ; and as the formula of the mineral is by no means definitely fixed, I requested Mr. Early to analyze by his method a portion of a particularly fine crystal which I obtained some time ago from the well-known Elba locality, our chief aim being to determine with precision the relative amounts of ferrous and ferric compounds present in the specimen.

The analysis was conducted in the following manner :—

1·54 grm. of the finely and recently powdered mineral were mixed with 20 cubic centims. of hydrofluoric acid (containing 20 per cent. of real acid) ; and the mixture was boiled for five minutes in a deep platinum crucible with a rather loosely fitting cover. 10 cubic centims. of diluted sulphuric acid (1 part to 2 of water) were then added, and the boiling continued for a few minutes. The contents of the crucible were then washed into a flask with air-free water, and the amount of iron in the ferrous condition determined as rapidly as possible by standard potassic permanganate solution. Another quantity of the mineral was acted upon by strong hydrochloric acid ; perfect decomposition was effected and a gelatinous

* Chemical News, October 9, 1874.

mass formed; the product was evaporated to dryness, and the silica separated in the usual way. The acid filtrate from the insoluble silica was then saturated with chlorine gas, and ammonia afterwards added in slight excess; the mixture produced was then boiled in a closely covered beaker in order to remove the excess of ammonia, the solution rapidly filtered, and the precipitate collected and ignited with the usual precautions and weighed. The product contained all the iron as ferric oxide, the alumina, the manganese as $Mn_3 O_4$, and a trace of silica. The silica was separated from this mixture by hydrochloric acid; and the filtrate was subjected to the double treatment with pure caustic soda for the separation of alumina. The iron and manganese were then separated by the baric-carbonate method. From the weight of iron thus found, that previously ascertained to be present in the ferrous state was deducted; the difference represented the weight of metal in the ferric condition. The filtrate from the first precipitate caused by ammonia had the calcium separated from it as oxalate; and the latter was determined in the usual way; the filtrate from the calcium precipitate was then evaporated to dryness and the residue heated to expel ammoniacal salts; the product of this treatment was dissolved with the aid of a few drops of hydrochloric acid, the magnesium separated by means of baric hydrate and estimated, while the alkalies in the filtrate were converted into chlorides and weighed, and the potassium separated by platinic chloride. No trace of lithium was detected in the mineral.

2·841 grms. of the freshly powdered and unaltered mineral were heated gradually to redness in a hard glass tube connected with a weighed chloride-of-calcium tube; a current of dry air was at the same time slowly drawn through the apparatus. The water collected weighed ·012 grm. = ·422 per cent only.

The percentage composition of the specimen analyzed by Mr. Early may be thus stated, when the metallic and other components are calculated as oxides:—

SiO_2	29·93
FeO	31·83
$Fe_2 O_3$	20·16
MnO	3·02
CaO	13·71
MgO	·30
$Al_2 O_3$	·36
$K_2 O$	·20
$Na_2 O$	·29
$H_2 O$	·42
	100·22

These data, when discussed in the usual way, give the following ratios :—

$$SiO_2 \ \ldots\ldots\ = \cdot 4983 = 3 \cdot 85 = 4 \cdot 00,$$
$$RO \ \ \ldots\ldots\ = \cdot 7431 = 5 \cdot 74 = 5 \cdot 96,$$
$$R_2O_3 \ldots\ldots\ = \cdot 1294 = 1 \cdot 00 = 1 \cdot 04 ;$$

or

$$R_2O_3, \ 6\,RO, \ 4\,SiO_2,$$

where $6\,RO = 4(Fe, Mn)O + 2CaO$ nearly, neglecting the small amount of alkalies.

As the water present in the particularly pure specimen of the mineral analyzed did not reach 0·5 per cent., it is difficult to admit, with Städeler, that it enters into the molecule of the compound ; I therefore prefer to regard Lievrite as an anhydrous silicate.

Mr. Early's analysis of the mineral agrees in all essential particulars with those of Rammelsberg and von Kobell, though both those distinguished observers found slightly more iron in the ferric condition. A comparison of the analyses of different specimens of Lievrite by Rammelsberg, von Kobell, Städeler*, and Early prove that there is little variation in the proportion of Fe_2^{VI} to Fe'' ; I am therefore disposed to regard the former as an essential constituent of the mineral, rather than as a product of the oxidation of a calcio-ferrous silicate. That the mineral oxidizes in time there can be no doubt ; but I have had a number of specimens of Lievrite under observation for nearly ten years, and though two of them were placed in a rather damp case, they suffered comparatively slight superficial oxidation.

If, then, we admit that Lievrite is essentially a dicalcic-ferroso-ferric silicate, we can assign to it the following symmetrical formula:—

This formula has at least the merit of indicating that the function of the ferric group is probably one of considerable importance, and that, so far from being regarded as an accidental constituent of the mineral, it ought to be considered one of the most important components of the molecule of the compound.

* See Dana's 'System of Mineralogy,' 5th edition, p. 296.

XL. *On Liquid Vortex-Rings.* By JOHN TROWBRIDGE, *S.D.,* *Assistant Professor of Physics, Harvard College* [*].

IT has often been observed by chemists that a drop of coloured liquid falling from a burette or a capillary tube into a liquid of different specific gravity, in which it can diffuse, assumes the form of a ring. Vortex motion, by the researches of Helmholtz, Thomson, and Maxwell, is now attracting so much attention that I have thought that a study of the general equations of motion of matter in connexion with a study of the rings would contribute to our knowledge of vortex movement. Professor W. B. Rogers published in the American Journal of Arts and Sciences for 1858 an extended paper on smoke rings and liquid rings, and described various methods of forming them. This paper seems to have been overlooked by recent investigators. It is a singular coincidence that Helmholtz should have published his great memoir on vortex movements in the same year that the article of Professor Rogers, which details purely the experimental side of the subject, appeared on the other side of the Atlantic. Professor Tait's method of forming smoke rings, which is also referred to by Sir William Thomson in his paper on vortex atoms, is now well known. The apparatus consists merely of a box closed at one end by a tightly stretched cloth, and having a circular hole of 6 or 8 inches diameter at the other. Clouds of sal-ammoniac vapour are generated inside the box; and rings are expelled by striking the stretched cloth with the hand. Sir William Thomson suggests that two such boxes, placed so that the rings might impinge on each other at any angle, would form a useful apparatus in studying the behaviour of such rings towards each other. At the conclusion of this paper several methods of studying liquid rings will be described.

When a drop of liquid falls from a small distance into a liquid of less density, in which it cannot diffuse, the conditions of its motion the instant after it strikes the surface of the liquid of less density are indicated by the general equation of strains [†]. " For each particle we have the component velocities u, v, w parallel to the fixed axes OX, OY, OZ. These have the following expressions,

$$u = \frac{d\alpha}{dt}, \quad v = \frac{d\beta}{dt}, \quad w = \frac{d\gamma}{dt},$$

x, y, z, t being independent variables, and α, β, γ functions of them. If the disturbed condition is so related to the initial condition that every particle of the body can pass from its

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[*] Communicated by the Author, having been presented at a Meeting of the American Academy of Arts and Sciences, January 14, 1877.

[†] Thomson and Tait's 'Natural Philosophy.'

initial to its disturbed position and strain by a translation and
a strain without rotation, *i. e.* if the three principal axes of the
strain at any point are lines of the substance which retain
their parallelism, we must have

$$\frac{d\beta}{dz} = \frac{d\gamma}{dy}, \quad \frac{d\gamma}{dx} = \frac{d\alpha}{dz}, \quad \frac{d\alpha}{dy} = \frac{d\beta}{dx}; \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

and if these equations are fulfilled the strain is non-rotational,
as specified; but these three equations express neither more nor
less than that $\alpha dx + \beta dy + \gamma dz$ is the differential of a function of
three independent variables." In other words we have a
strain-potential; and in the case of strains rotation is incon-
sistent with the existence of a strain-potential. The forces
which solicit the particles of a drop when it rests upon the
liquid of less density, in which it cannot diffuse, are evidently
their mutual attraction, a force arising from the superficial
tension of the liquid upon which the drop rests, and a force
arising from gravitation. It is evident from a consideration
of these forces that, after the drop has suffered a strain at the
liquid surface, every particle of the drop cannot pass from its
initial position to the next following position by a translation
and a strain without rotation; for the drop tends to return
from a shape approaching an oblate spheroid to that of a
sphere. Then equations (1) do not hold, and a strain-poten-
tial does not exist, and the drop must rotate. This rotation
is not in general of the ring-form. If, on the contrary, the
drop of liquid can diffuse itself in the liquid through which it
falls, each particle, with the velocities u, v, w, is solicited at
the moment of impact by a superficial tension, by the force of
gravitation, and, on account of the tendency to diffuse, the
forces of attraction which tend to make the non-diffusing drop
reassume the spherical shape are very small. To assume that
each particle of the drop, in the next state to that which it
assumes on striking the free surface of the liquid, is translated
without rotation, is to assume that each particle is compelled
to move in restrained limits which do not exist.

If we follow the notation of Poisson[*] and Helmholtz[†], we
shall have for the general equations of motion of an internal
particle of a liquid,

$$\left.\begin{array}{l}
X - \dfrac{1}{h} \cdot \dfrac{d\rho}{dx} = \dfrac{du}{dt} + u\dfrac{du}{dx} + v\dfrac{du}{dy} + w\dfrac{du}{dz}, \\[2mm]
Y - \dfrac{1}{h} \cdot \dfrac{d\rho}{dy} = \dfrac{dv}{dt} + u\dfrac{dv}{dx} + v\dfrac{dv}{dy} + w\dfrac{dv}{dz}, \\[2mm]
Z - \dfrac{1}{h} \cdot \dfrac{d\rho}{dz} = \dfrac{dw}{dt} + u\dfrac{dw}{dx} + v\dfrac{dw}{dy} + w\dfrac{dw}{dz},
\end{array}\right\} \quad \cdot \quad \cdot \quad (2)$$

[*] *Traité de Mécanique.* [†] Crelle's *Journal,* vol. lv. 1858.

$$\frac{dh}{dt} + u\frac{dh}{dx} + v\frac{dh}{dy} + w\frac{dh}{dz} = \frac{\delta h}{\delta t}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

in which ρ is the pressure at the point x, y, z, X, Y, Z are the components of the external forces acting on a unit of mass, and h is the density at the point x, y, z. The forces X, Y, Z are considered to have a potential V, so that

$$X = \frac{dV}{dx}, \quad Y = \frac{dV}{dy}, \quad Z = \frac{dV}{dz}; \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

and the velocities u, v, w a velocity-potential ϕ, so that

$$u = \frac{d\phi}{dx}, \quad v = \frac{d\phi}{dy}, \quad w = \frac{d\phi}{dz}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (6)$$

or

$$u dx + v dy + w dz = d\phi ;$$

and ϕ satisfies the equation

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (7)$$

which is equation (4) under the conditions expressed in equation (6). We must also have

$$\frac{du}{dy} = \frac{dv}{dx}, \quad \frac{dv}{dz} = \frac{dw}{dy}, \quad \frac{dw}{dx} = \frac{du}{dz}, \quad \cdot \quad \cdot \quad \cdot \quad (8)$$

equations similar to equations (1). Helmholtz has shown that in the case of rotation of a fluid-element, equations (8) become

$$\left.\begin{aligned} \frac{dv}{dz} - \frac{dw}{dy} &= 2\xi, \\ \frac{dw}{dx} - \frac{du}{dz} &= 2\eta, \\ \frac{du}{dy} - \frac{dv}{dx} &= 2\zeta; \end{aligned}\right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (9)$$

" and therefore the existence of a velocity-potential is inconsistent with the existence of rotation of the fluid-element." We have also seen from the equations of strains that the existence of a strain-potential is inconsistent with the rotation of a material particle ; and therefore, from the conditions of impact, the particles of a drop of diffusing material are in a condition to rotate. Let us now see if vortex movement can arise in a liquid from variation of density and pressure. Following

Helmholtz's notation, we have, if ψ is a function of x, y, z, t,

$$\frac{\delta \psi}{\delta t} = \frac{d\psi}{dt} + u\frac{d\psi}{dx} + v\frac{d\psi}{dy} + w\frac{d\psi}{dz}. \quad . \quad . \quad . \quad (10)$$

Calling ξ, η, ζ the components of the angular velocity, we can obtain their variations with the time by substituting them in succession in equation (10). If we eliminate X, Y, Z from equations (2) by the help of equations (5), and introduce the values of ξ, η, ζ from equations (9), supposing that h is a function of x, y, z, t, we obtain

$$\frac{\delta \xi}{\delta t} = -\left(\frac{dv}{dy} + \frac{dw}{dz}\right) + \eta\frac{dv}{dx} + \zeta\frac{dw}{dx} + \frac{1}{2h^2}\left(\frac{dh}{dz}\frac{d\rho}{dy} - \frac{dh}{dy}\frac{d\rho}{dz}\right), (11)$$

and similar expressions for the variations of η and ζ. It will be seen that in this case terms of the form

$$\frac{1}{2h^2}\left(\frac{dh}{dz}\frac{d\rho}{dy} - \frac{dh}{dy}\frac{d\rho}{dz}\right),$$

independent of ξ, η, ζ, and depending upon the variations of h and ρ, enter into the expressions for the variations of the angular velocities; and therefore a vortex movement is to be expected in a process of diffusion by a variation of density and pressure without initial angular velocities. This condition can be shown experimentally by dropping a solution of one of the aniline colours into a mixture of glycerine and water. The original ring, after ceasing to move downward in the mixture, breaks up gradually into segments, which in their turn slowly assume the ring form. A mixture of water and glycerine is not necessary; peculiar cup-like figures, indicating the first stage of vortical movement, can be seen whenever a thin stratum of one liquid slowly diffuses itself through another liquid of different density.

By a consideration of the equations

$$\epsilon \xi + (u_1 - u)dt = \epsilon\left(\xi + \frac{\delta \xi}{\delta t}\ dt\right),$$

$$\epsilon \eta + (v_1 - v)dt = \epsilon\left(\eta + \frac{\delta \eta}{\delta t}\ dt\right),$$

$$\epsilon \zeta + (w_1 - w)dt = \epsilon\left(\zeta + \frac{\delta \zeta}{\delta t}\ dt\right),$$

given by Helmholtz, from which he draws the conclusion that " each vortex-line remains continually composed of the same elements of fluid and swims forward with them in the fluid," We see, on introducing the new expressions which we have

found for $\frac{\delta\xi}{\delta t}, \frac{\delta\eta}{\delta t}, \frac{\delta\zeta}{\delta t}$, equations (11), that we approach nearer and nearer to this conclusion when the variations of h are smaller and smaller. Obviously we should therefore obtain the most perfect liquid rings when the drop and the liquid in which the motion takes place are composed of the same liquid. A drop of water falling into water must form a more perfect ring than that formed by a drop of any coloured liquid of greater density than water; and every drop of water falling into water from a height not too great must necessarily form a vortex-ring.

The formation of liquid rings is as fascinating and as simple an occupation as blowing soap-bubbles. All liquid drops falling from such a height that the surface of the liquid in which they are about to diffuse is not too much disturbed to enable the drop to be acted upon symmetrically by the forces at the free surface, will form rings if too great differences of density do not exist. That a drop of pure water will descend through the same liquid in a vortex ring can be shown experimentally by covering the free surface of the water with a fine light powder. Particles of the powder will be carried down by the drop and will be seen to rotate in a ring-shape far below the surface. This fact can be shown also by the employment of any of the aniline colours which are solvent in water, the drop consisting of a coloured solution whose density does not differ sensibly from that of water. The method which I have employed to produce the rings consists merely of a small glass tube, slightly smaller at one end than the other. A piece of cotton is wedged in the tube; and flexible rubber tubing is attached to the larger end. With the aid of the mouth one can fill this tube with liquid and eject it in drops at pleasure. A modification of the same apparatus enables one to form the rings beneath the surface of the liquid. With a bent tube one can send the rings through a liquid in any desired direction. By means of a three-way glass joint and a small india-rubber bag, one can send by the same impulse two rings whose paths make any desired angle with each other. By dropping a suitable liquid through a layer of oil superincumbent on water and glycerine, one can study the rings formed by the sudden bursting of the enveloping film of oil when the drop strikes the layer of water and glycerine. A simple method of studying the mutual behaviour of half vortex-rings is to illuminate, by means of a gaslight, the bottom of a flat white porcelain dish filled with water, and to observe the shadows on the bottom of the dish of the half vortex-rings formed by the movement of two spatula along the surface. It can be readily seen

by this simple means that a half vortex-ring moving near another in a parallel path with a less velocity tends to follow in the path of the first ; and that two equal half vortex-rings moving in opposite directions along the same path separate into two vortices which move at right angles to the path of the original vortices.

Whenever a stratum of dense vapour is formed in the higher regions of the atmosphere, it is evident from the preceding discussion that it will tend to descend in the form of a vortex-ring if disturbing forces are not present. The results of this paper are as follows :—

1. An analogy between the strain-potential and the velocity-potential is indicated.

2. It is shown that the formation of liquid rings is a necessary consequence of the fundamental equations of strain and of hydrodynamics, and that a difference of density is not necessary to produce them.

3. Vortices can and do arise in certain processes of diffusion.

4. Simple methods of studying vortex motions in liquids are given.

Physical Laboratory, Harvard College,
    Cambridge, U. S. A.

XLI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 235.]

June 15, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

THE following papers were read :—
"The Action of Light on Selenium." By Prof. W. G. Adams, F.R.S., and Mr. R. E. Day.

The paper contains an account of a series of experiments which have been carried on during the past year, and which have had for their object the investigation of the electrical behaviour of selenium, especially as regards its sensitiveness to light. The first part contains a short summary of the results obtained by Professor Adams, which have been communicated to the Society[*].

It has been already shown that the action is due principally, if not entirely, to those rays of the spectrum which are luminous, and that the ultra-red or the ultra-violet rays have little or no effect ; also that the intensity of the action depends on the illuminating power of the light, being directly as the square root of that illuminating power.

It was also observed that with the same piece of selenium at the same temperature, the resistance diminished as the battery-power

               * See Phil. Mag. 1876, vol. i. pp. 155, 322.

was increased. Also it was found that the electrical resistance of the rod of selenium was different for currents going through it in opposite directions. Thus if two platinum wires be melted into the selenium at two points, A and B, and the resistance of the selenium be balanced by the Wheatstone's-bridge arrangement, the positive pole of the battery being connected to the electrode A, then, on reversing the current so that the negative pole of the battery was now connected to the electrode A, the numerical value of the balancing resistance was always found to be different from that previously obtained.

If the electrical conductivity of selenium followed the ordinary law of metallic conduction, this would not be the case ; and hence it seemed probable that a careful investigation of these points would lead to some important results.

In the experiments recorded in this paper, the objects we have had especially in view have been :—

(i) To examine the character of the electrical conductivity of selenium when kept in the dark.

(ii) To determine whether light could actually generate an electric current in the selenium.

Several pieces of selenium were prepared as follows :—A small piece varying from a quarter of an inch to an inch in length was broken off a stick of vitreous selenium. A platinum wire was then taken and bent round into a small ring at one end, and the remainder of the wire turned up at right angles to the plane of this ring. The rings of two such wires were then heated in the flame of a spirit-lamp, and pressed into the ends of the little cylinder of selenium, thus forming platinum electrodes. The whole was then annealed. After annealing, copper wires were soldered on to the platinum electrodes, and the selenium was then inclosed in a piece of glass tube, the electrodes being passed through corks fixed at the ends of the tube. A numbered label was then attached to one of the electrodes ; and this was then always described as the " marked " electrode.

The method of annealing which we have found to give the best results is very simple. A large iron ball is heated to a bright red heat, and then placed in a large iron bowl of sand ; the sand is then heaped up all over the ball, and left for an hour. The ball is then taken out, and the selenium, wrapped up in paper, is put into the hot sand and left there for twenty-four hours. On removing it from the sand its appearance has generally changed from a bright glassy character to a dull slate-coloured one ; and when this is the case its conductivity is generally very good.

In most of our experiments it was important to know what was the *direction* of the current in any particular case ; and we therefore decided to call those currents direct or positive currents when the positive electrode of the battery was connected with the *marked* electrode of the selenium plate under examination. In order to be able to reverse the current with respect to the selenium without affecting any other portion of the circuit, the

ends of the wire electrodes of the selenium were made to dip into two little mercury-cups fixed on a plate of ebonite, and then were connected to the binding-screws of the Wheatstone-bridge arrangement. Thus by reversing the position of the electrodes the direction of the current through the selenium was reversed. The positive direction of the current was always determined at the commencement of each series of experiments by means of a delicately suspended magnetic needle.

A few preliminary experiments were made to determine whether the change of resistance with change of direction of the current had any connexion with the position of the selenium or the direction of the current with respect to the magnetic meridian. No such connexion was found to exist.

From the results obtained from a great many experiments made to determine the diminution of resistance with increased battery-power, and the change of resistance with a change of the direction of the current, the following conclusions were drawn :—

(1) That on the whole there is a general diminution of resistance in the selenium as the battery-power is increased.

(2) The first current through the selenium, if a strong one, causes a permanent *set* of the molecules, in consequence of which the passage of the current through the selenium during the remainder of the experiments is more resisted in that direction than it is when passing in the opposite direction.

(3) The passage of the current in any direction produces a *set* of the molecules which facilitates the subsequent passage of a current in the opposite, but obstructs one in the same direction. Hence when two currents are sent through successively, after a very small interval, in the same direction, the resistance observed in the second case, even with the higher battery-power, is often equal to or greater than it was before.

The results of these experiments seeming to indicate that the conductivity of selenium is electrolytic, a number of experiments were undertaken in order to discover whether after the passage of an electric current through a piece of selenium any distinct evidence of polarization could be detected. It was then found that, after passing the current from a voltaic battery for some time through the selenium, and after having disengaged the electrodes from the battery and connected them with a galvanometer, a current, in some cases of considerable intensity, in the opposite direction to that of the original battery-current, passed through the galvanometer. This proved that the passage of the battery-current sets up polarization in the selenium.

All the results hitherto described were obtained with the selenium kept in the dark.

We then tried to discover whether on exposing the selenium to light during the passage of the polarization-current any change in the intensity of that current would be produced. We found that in several cases there was a distinct change : in most instances the action of the light assisted the passage of the current ; but in

one case we found that the effect of light was not only to bring the deflection of the galvanometer-needle down to zero, but also to send it up considerably on the other side.

Here there seemed to be a case of light actually producing an electromotive force within the selenium, which in this case was opposed to and could overbalance the electromotive force due to polarization.

The question at once presented itself as to whether it would be possible to *start a current in the selenium merely by the action of light*. Accordingly the same piece of selenium was connected directly with the galvanometer. While unexposed there was no action whatever. On exposing the tube to the light of a candle, there was at once a strong deflection of the galvanometer-needle. On screening off the light the deflection came back at once to zero.

This experiment was repeated in various ways and with light from different sources, the results clearly proving that by the action of light alone we could start and maintain an electrical current in the selenium.

All the pieces of selenium hitherto used had repeatedly had electrical currents passing through them; and it therefore seemed desirable to examine the effect of exposure to light on pieces of selenium which had never before had an electrical current sent through them.

Accordingly three pieces were prepared, as nearly alike as possible, and were annealed. Two of them were found on trial to be sensitive to light; that is to say, light impinging on them produced an electrical current; the third piece, however, showed no signs of sensitiveness. Hence it appears that three pieces which were made up from the same stick, which are of the same length, and were annealed at the same time, may, owing to some slight difference in their molecular condition, be very different as to their relative sensitiveness to the action of light.

In the experiments by which the above results were obtained, the piece of selenium under examination had always been exposed as a whole to the influence of the light; so that it was not possible to tell whether any one part of a piece was more sensitive than any other.

In order to examine into this point more fully, we used the limelight, and then by means of a lens the light was brought to a focus on the particular portion of the selenium plate which was to be tested. A glass cell containing water, and having parallel sides, was interposed in the path of the beam, so as to assist in absorbing any obscure heat-rays.

The results of these experiments proved conclusively the following points :—

(1) That pieces of annealed selenium are in general sensitive to light, *i. e.* that under the action of light a difference of potential is developed between the molecules, which under certain conditions can produce an electric current through the substance.

(2) That the sensitiveness is different at different parts of the same piece.

(3) That in general the direction of the current is from the less towards the more illuminated portion of the selenium, but that, owing to accidental differences in molecular arrangement, this direction is sometimes reversed.

The currents produced in the selenium by the action of light do not resemble the thermoelectric currents due to heating of the junctions between the platinum electrode and the selenium; for in many cases the current produced was most intense when the light was focused on points of the selenium not coinciding with the junctions; also the current was produced suddenly on exposure, and on shutting off the light the needle *at once* fell to zero; the gradual action due to gradual cooling was entirely wanting.

When the light fell upon a junction, the current passed from the selenium to the platinum through the junction, which is not in accordance with the place assigned to selenium in the thermoelectric series of metals.

Experiments were next undertaken in order to examine what effect would be produced on the strength of a current which was passing through a piece of selenium in the dark when a beam of light was allowed to fall upon it.

The results obtained from these experiments were as follows :—

With pieces of selenium of low resistance and with a weak current passing through them—

(1) When light falls on the end of the selenium at which the current from the positive pole of the battery is entering the metal, it *opposes* the passage of the current.

(2) When light falls on the end of the selenium at which the current is leaving the metal, it *assists* the passage of the current.

With pieces of selenium of a high resistance we found that in all cases the action of light tended to facilitate the passage of the battery-current, whichever was its direction.

We also found that in those pieces which appeared so little sensitive to light that no independent current was developed in them by exposure, yet, when a current due to an external electromotive force was passing through them, the exposure to light facilitated the passage of the current.

The results of the experiments described in this paper furnish a possible explanation of the character of the action which takes place when light falls upon a piece of selenium which is in a more or less perfect crystalline condition.

When a stick of vitreous selenium has been heated to its point of softening, if it were possible to cool the whole equally and very slowly, then the whole of the molecules throughout its mass would be able to take up their natural crystalline positions, and the whole would then be in a perfectly crystalline state, and would conduct electricity and heat equally well throughout its mass. But from the nature of the process it is evident that the outer layers will

cool the most rapidly, and we shall have, in passing from the out-
side to the centre, a series of strata in a more and more perfect
crystalline condition.

Light, as we know in the case of some bodies, tends to promote
crystallization, and, when it falls on the surface of such a stick of
selenium, probably tends to promote crystallization in the exterior
layers, and therefore to produce a flow of energy from within out-
wards, which under certain circumstances appears, in the case of
selenium, to produce an electric current.

The crystallization produced in selenium by light may also ac-
count for the diminution in the resistance of the selenium when a
current from a battery is passing through it ; for in changing to
the crystalline state selenium becomes a better conductor of elec-
tricity.

" On the Application of the Principle of Reciprocity to Accous-
tics."   By Lord Rayleigh, F.R.S.

In a memoir published some years ago by Helmholtz (Crelle,
Bd. lvii.) it was proved that if a uniform frictionless gaseous
medium be thrown into vibration by a simple source of sound of
given period and intensity, the variation of pressure is the same
at any point B when the source of sound is at A as it would have
been at A had the source of sound been situated at B, and that
this law is not interfered with by the presence of any number of
fixed solid obstacles on which the sound may impinge.

A simple source of sound is a point at which the condition of
continuity of the fluid is broken by an alternate introduction and
abstraction of fluid, given in amount, and periodic according to
the harmonic law.

The reciprocal property is capable of generalization so as to
apply to all acoustical systems whatever capable of vibrating about
a configuration of equilibrium, as I proved in the Proceedings
of the Mathematical Society for June 1873, and is not lost even
when the systems are subject to damping, provided that the fric-
tional forces vary as the first power of the velocity, as must always
be the case when the motion is small enough.   Thus Helmholtz's
theorem may be extended to the case when the medium is not
uniform, and when the obstacles are of such a character that they
share the vibration.

But although the principle of reciprocity appears to be firmly
grounded on the theoretical side, instances are not uncommon in
which a sound generated in the open air at a point A is heard
at a distant point B, when an equal or even more powerful sound
at B fails to make itself heard at A ; and some phenomena of
this kind are strongly insisted upon by Prof. Henry in opposition
to Prof. Tyndall's views as to the importance of " acoustic clouds "
in relation to the audibility of fog-signals.   These observations
were not, indeed, made with the simple sonorous sources of theory ;
but there is no reason to suppose that the result would have been
different if simple sources could have been used.

In experiments having for their object the comparison of sounds heard under different circumstances there is one necessary precaution to which it may not be superfluous to allude, depending on the fact that the audibility of a particular sound depends not only upon the strength of that sound, but also upon the strength of other sounds which may be heard along with it. For example, a lady seated in a closed carriage and carrying on a conversation through an open window in a crowded thoroughfare will hear what is said to her far more easily than she can make herself heard in return ; but this is no failure in the law of reciprocity.

The explanation of his observations given by Henry depends upon the peculiar action of wind, first explained by Prof. Stokes. According to this view a sound is ordinarily heard better with the wind than against it, in consequence of a curvature of the rays. With the wind a ray will generally be bent downwards, since the velocity of the air is generally greater overhead than at the surface, and therefore the upper part of the wave-front tends to gain on the lower. The ray which ultimately reaches the observer is one which started in some degree upwards from the source, and has the advantage of being out of the way of obstacles for the greater part of its course. Against the wind, on the other hand, the curvature of the rays is upwards, so that a would-be observer at a considerable distance is in danger of being left in a sound-shadow.

It is very important to remark that this effect depends, not upon the mere existence of a wind, but upon the velocity of the wind being greater overhead than below. A uniform translation of the entire atmosphere would be almost without effect. In particular cases it may happen that the velocity of the wind diminishes with height, and then sound is best transmitted *against* the wind. Prof. Henry shows that several anomalous phenomena relating to the audibility of signals may be explained by various suppositions as to the velocity of the wind at different heights. When the distances concerned are great, comparatively small curvatures of the ray may produce considerable results.

There is a further possible consequence of the action of wind (or variable temperature), which, so far as I know, has not hitherto been remarked. By making the velocity a suitable function of height it would be possible to secure an actual convergence of rays in a vertical plane upon a particular station. The atmosphere would then act like the lens of a lighthouse, and the intensity of sound might be altogether abnormal. This may perhaps be the explanation of the extraordinary distances at which guns have sometimes been heard.

The difference in the propagation of sound against and with the wind is no exception to the general law referred to at the beginning of this communication ; for that law applies only to the vibrations of a system about a configuration of equilibrium. A motion of the medium is thus excluded. But the bending of the sound-ray due to a variable temperature, to which attention has

been drawn by Prof. Reynolds, does not interfere with the application of the law.

An experiment has, however, been brought forward by Prof. Tyndall, in which there is an apparent failure of reciprocity not referable to any motion of the medium[*]. The source of sound is a very high-pitched reed mounted in a short tube and blown from a small bellows with which it is connected by rubber tubing. The variation of pressure at the second point is made apparent by means of the sensitive flame, which has been used by Prof. Tyndall with so much success on other occasions. Although the flame itself, when unexcited, is 18 to 24 inches high, it was proved by a subsidiary experiment that the root of the flame, where it issues from the burner, is the seat of sensitiveness. With this arrangement the effect of a cardboard or glass screen interposed between the reed and the flame was found to be different, according as the screen was close to the flame or close to the reed. In the former case the flame indicated the action of sound, but in the latter remained uninfluenced. Since the motion of the screen is plainly equivalent to an interchange of the reed and flame, there is to all appearance a failure in the law of reciprocity.

At first sight this experiment is difficult to reconcile with theoretical conclusions. It is true that the conditions under which reciprocity is to be expected are not very perfectly realized, since the flame ought not to be moved from one position to the other. Although the seat of sensitiveness may be limited to the root of the flame, the tall column of highly heated gas might not be without effect; and in fact it appeared to me possible that the response of the flame, when close to the screen, might be due to the conduction of sound downwards along it. Not feeling satisfied, however, with this explanation, I determined to repeat the experiment, and wrote to Prof. Tyndall, asking to be allowed to see the apparatus. In reply he very kindly proposed to arrange a repetition of the experiment at the Royal Institution for my benefit, an offer which I gladly accepted.

The effect itself was perfectly distinct, and, as it soon appeared, was not to be explained in the manner just suggested, since the response of the flame when close to the screen continued, even when the upper part of the heated column was protected from the direct action of the source by additional screens interposed. I was more than ever puzzled, until Mr. Cottrell showed me another experiment, in which, I believe, the key of the difficulty is to be found.

When the axis of the tube containing the reed is directed towards the flame, situated at a moderate distance, there is a distinct and immediate response; but when the axis is turned away from the flame through a comparatively small angle, the effect ceases, although the distance is the same as before, and there are no obstacles interposed. If now a card-board screen is held in the prolongation of the axis of the reed, and at such an angle as to

[*] Proceedings of the Royal Institution, January 1875; also Prof. Tyndall's work on Sound, 3rd edition,

reflect the vibrations in the direction of the flame, the effect is again produced with the same apparent force as at first.

These results prove conclusively that the reed does not behave as the simple source of theory, even approximately. When the screen is close (about 2 inches distant) the more powerful vibrations issuing along the axis of the instrument impinge directly upon the screen, are reflected back, and take no further part in the experiment. The only vibrations which have a chance of reaching the flame, after diffraction round the screen, are the comparatively feeble ones which issue nearly at right angles with the axis. On the other hand, when the screen is close to the flame, the efficient vibrations are those which issue at a small angle with the axis, and are therefore much more powerful. Under these circumstances it is not surprising that the flame is affected in the latter case and not in the former.

The concentration of sound in the direction of the axis is greater than would have been anticipated, and is to be explained by the very short wave-length corresponding to the pitch of the reed. If, as is not improbable, the overtones of the note given by the reed are the most efficient part of the sound, the wave-length will be still shorter and the concentration more easy to understand*.

The reciprocal theorem in its generalized form is not restricted to simple sources, from which (in the absence of obstacles) sound would issue alike in all directions ; and the statement for *double sources* will throw light on the subject of this note. A double source may be thus defined :—Conceive two equal and opposite simple sources, situated a short distance apart, to be acting simultaneously. By calling the two sources opposite, it is meant that they are to be at any moment in opposite phases. At a moderate distance the effects of the two sources are antagonistic and may be made to neutralize one another to any extent by diminishing the distance between the sources. If, however, at the same time that we diminish the interval, we augment the intensity of the single sources, the effect may be kept constant. Pushing this idea to its limit, when the intensity becomes infinite and the interval vanishes, we arrive at the conception of a double source having an axis of symmetry coincident with the line joining the single sources of which it is composed. In an open space the effect of a double source is the same as that communicated to the air by the vibration of a solid sphere whose centre is situated at the double point and whose line of vibration coincides with the axis, and the intensity of sound in directions inclined to the axis varies as the square of the cosine of the obliquity.

The statement of the reciprocal theorem with respect to double sources is then as follows:—If there be equal double sources at two points A and B, having axes A P, B Q respectively, then the

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* July 13.—I have lately observed that the flame in question is extremely sensitive to one of Mr. F. Galton's whistles, which gives notes near the limits of ordinary hearing.

velocity of the medium at B resolved in the direction B Q due to the source at A is the same as the *velocity* at A resolved in the direction A P due to the source at B. If the waves observed at A and B are sensibly plane, and if the axes A P, B Q are equally inclined to the waves received, we may, in the above statement, replace " velocities " by " pressures," but not otherwise.

Suppose, now, that equal double sources face each other, so that the common axis is A B, and let us examine the effect of interposing a screen near to A. By the reciprocal theorem, whether there be a screen or not, the velocity at A in direction A B due to B is equal to the velocity at B in direction A B due to A. The waves received at B are approximately plane and perpendicular to A B, so that the relation between the velocity and pressure at B is that proper to a plane wave; but it is otherwise in the case of the sound received at A. Accordingly the reciprocal theorem does not lead us to expect an equality between the pressures at A and B, on which quantities the behaviour of the sensitive flames depends. On the contrary, it would appear that the pressure at A corresponding to the given velocity along A B should be much greater than in the case of a plane wave, and then the relative advantage of the position A would be explained.

It will be seen that, if the preceding arguments are correct, Prof. Tyndall's experiment does not bear out the conclusions that he has based upon it with respect to the observations of the French Commission at Villejuif and Montlhéry. No acoustic clouds could explain the failure of reciprocity then observed; and the more probable hypothesis that the effect was due to wind is not inconsistent with the observation that the air (at the surface) was moving in the direction against which the sound was best heard.

Further experiments on this subject are very desirable.

" On Supersaturated Saline Solutions." By J. G. Grenfell, B.A., F.G.S.

In making experiments on the sensitiveness of supersaturated solutions to air and greasy surfaces, I was much annoyed by the solutions so frequently crystallizing on the removal of the cotton-wool, as this necessitated boiling the flask again and waiting till it was cool. I noticed that frequently part of the cotton-wool adhered to the mouth of the flask; and it struck me that, in removing this, some fibres must get detached and fall in, carrying with them in all probability crystals of the salt. I soon convinced myself that this was the case, and that cotton-wool is perhaps the worst material that could be chosen for covering these solutions. I now always use paper or tinfoil; and I find that these can be removed many times from the same solution without inducing crystallization. I then found that even the most sensitive solutions could be taken up in a clean glass tube and dropped on a clean glass plate without crystallizing, and that they will remain liquid exposed to the air for a very long time, often, in fact, till they dry up by evaporation in modified forms. Twenty drops on a plate give twenty experiments on the effect of air, clean and

unclean surfaces, and evaporation; then the plate is cleaned, and more drops are taken from the original solution till this is used up. The trouble of boiling is thus reduced to a minimum, and the drops can be put upon all kinds of surfaces to test their activity. The slow growth of the modified salts can be watched for hours; and their forms are sometimes peculiar: thus sulphate of soda often gives a single, square, flat pyramid, or a broad well-shaped prism, or occasionally small octahedra round the edge of the drop. The pyramids and prisms change to opaque white when touched, and are apparently the 7-atom salt; the octahedra do not change, and are evidently the anhydrous salt. This fact is interesting, from its supporting the view that it is the anhydrous salt which is in solution.

Or, again, a plate with drops may be dried over calcium chloride; and this sometimes modifies the results, as in the case of ammonia alum. This salt, when allowed to evaporate in air, generally forms a shining semitransparent film of greenish colour with a depression at the top, in which is often a circular opening, while inside small globular concretions of a dull, opaque, milky white colour are formed; these will remain moist inside for a couple of days or more. When touched with the normal salt, the whole drop becomes brilliant opaque white, quite dry, and apparently increases in volume, as the crust often breaks up and curls outwards.

This modified salt is apparently new. I put some drops over calcium chloride: no film was formed, but the drops crystallized very slowly in the globular forms mixed with little, clear, flat, very thin pointed plates which reminded me much of a particular form of aluminium sulphate. When dry all the drops were brilliant opaque white, and retained a good deal of water.

Potash alum forms similar films and globular masses. The mother-liquor of the ammonia alum sometimes slowly deposits short, fine, silky needles with a faint milky tinge and small globular masses. I have only recently adopted the method of using drops, and have not much leisure for working; but the field is so wide, and the results already obtained have such an important bearing on the theory of the crystallization of these solutions, that I have ventured to put them forward in their present incomplete state.

The most commonly received theory is that of which M. de Gernez is the most prominent advocate—that only a crystal of the same salt causes crystallization, and that these are introduced by the air, which is a vast storehouse of crystals of all kinds.

The following experiments seem to support the crystal theory; but at the same time they clearly show that the quantity of salts present in the atmosphere is indefinitely less than we have hitherto been led to suppose, and, in fact, they bring that quantity down within the limits of ordinary probability.

1. Put drops of a very strong solution of sulphate of soda on a plate on my laboratory table; waved a newspaper over them for some time, producing a strong current of air: most of them did

not crystallize, and one slowly dried up in octahedra. I have repeatedly of late boiled sulphuric acid in the laboratory, so that there can be no lack of sodium sulphate in the dust.

2. Drew a strong current of air over drops of sodium sulphate in a glass tube : inactive.

3. Drops of sodium sulphate put upon the leaves of many plants in my garden. They slowly evaporated, giving the 7-atom salt. The leaves were covered with dust, as the garden opens on to a road, and the weather has been hot and dry ; we are not far from Bristol, so we might expect to find sulphates.

4. Carried sodium sulphate to an upper room ; drops on the wash-hand stand, on the window-sill inside and out, on the iron bars outside : all inactive. Washed my hands and spread a drop with the finger on the window-sill, inside : inactive. Three drops crystallized on the mantlepiece, and one on the window-sill. Several drops on the window-frame evaporated as 7-atom salt.

5. Potash alum on a window-sill outside gave a modified film.

6. Sodium acetate put upon the cork of a large bottle which had stood for two years untouched in my laboratory. The drops were quite thick with dust, but remained liquid for more than 24 hours.

7. Other drops of the same put on the floor of the laboratory, on the dusty corners of the shelves, on paper, on every place and kind of surface I could find : remained liquid in all cases.

8. Spread a number of drops of the same on a glass plate, covering nearly the whole of it. Made about half crystallize. Left them exposed for three days; they remained liquid, though the normal salt effloresces slightly.

9. Ammonia alum : many drops on a glass plate ; they formed films by evaporation; made a good many crystallize, when they broke open, early in the day : carried them out in a high wind to the house of a neighbour, and brought them back ; then late at night put a number of fresh drops on the plate, and several of them remained liquid all night.

10. Sodium carbonate is not affected by any surface in my laboratory. I have spread a drop over a dirty glass plate so as to cover a good many square inches, and it slowly evaporated, giving crystals. Drops on the floor, shelves, bottles, &c. of the laboratory invariably remain liquid.

I could give many other instances, but these are sufficient to show that the air does not ordinarily contain these salts, and that it does not readily catch them up and deposit them on all kinds of surfaces ; and yet these salts are remarkably sensitive to crystals of the same kind. The effect of using cotton-wool is a good example of this. Another is this :—Touched a crystallized drop of sodium acetate with a pin ; passed the pin repeatedly through my coat : active at once. After touching a crystal the finger needs to be washed carefully. Again, sodium sulphate crystallizes almost invariably on any dirty surface in my laboratory, and ammonia alum generally. Even the sodium acetate crystallizes at times when I am at work with the same salt close by.

Sodium sulphate crystallizes generally on a clean plate exposed in my laboratory as 10-atom salt, whilst if protected by an inverted beaker it dries up by evaporation, forming the modified salt. So, again, I have had two drops of sodium sulphate liquid all night, and both crystallize within ten minutes of my entering the room in the morning. In my bedroom, however, I left a test-tube containing this solution open all night with the pipette on the mantlepiece. In the morning the solution had not crystallized, while the end of the pipette was covered with a white incrustation, which was inactive in the liquid. The incrustation was again left to dry up, and then contained plenty of water, being evidently the 7-atom salt.

For sodium acetate and carbonate it is quite useless to have any cover on the flask or test-tube which contains them, and also for the sulphate in an ordinary room. Care must be taken that crystals are not formed near the mouth of the tube, so as to fall in ; but that is the only precaution necessary. Carbonate of soda by evaporation becomes oily like sodium and potassium acetates. I have not yet investigated the composition of the films and crystals which these solutions deposit.

Normal sodium acetate when heated leaves a white mass which deliquesces, forming a strongly supersaturated solution. The anhydrous sulphate also forms a supersaturated solution when added to water, as De Coppet pointed out.

I touched a drop of the acetate with the point of a penknife ; a little drop crystallized on the penknife, but the drop itself did not. I then repeatedly touched the surface of the drop rapidly with the solidified part and obtained a little rod, formed of separate layers and nearly $\frac{1}{2}$ inch long. At last the rod broke in the drop which instantly crystallized. I have repeated this with carbonate of soda. The fact is interesting as showing how very local the crystallizing force is. Faraday had an idea that this force might possibly be transferred by wires ; but I have poured out part of a solution which was crystallizing into a test-tube, where it remained supersaturated.

Professor Tomlinson has long maintained with great ingenuity the theory that the cause of crystallization in these solutions is adhesion. To a surface covered with a film of greasy matter the salt adheres, while the liquid does not, and therefore separation follows. I do not think that theory can be sustained in the presence of the following facts :—

1. Rubbed the finger on the palm of the hand, and took up solution of alum from a drop, and deposited on another part of the same plate : inactive.

2. Rubbed oil on the palm of the hand, and repeated : again inactive.

3. Smeared oil over a glass plate : inactive to drops of alum.

4. Rubbed oil on the finger ; took up some sodium carbonate, and rubbed it hard on the plate : inactive.

5. Repeated this with sodium acetate.

The mere fact, however, that the salts are, as a rule, perfectly insensible to every kind of surface, wood, paint, paper, glass, and dust of all kinds, seems to me fatal to this theory.

A solution of one part of normal sodium sulphate in about six of sulphuric acid possesses some curious properties. This solution, which sets quite firm, can be kept for a week in an open beaker, so that the air apparently has no crystals to introduce; and yet when dropped on to a dirty surface in my laboratory it more often crystallizes than not. It is thus much more sensitive than an aqueous solution of sodium carbonate or acetate. The crystals are apparently a hydrate of the hyperacid salt $NaH_3(SO_4)_2$; and it is almost inconceivable that the dust should contain crystals of this salt. It is extremely deliquescent, and the excess of acid should certainly be taken up by the dust, and very often by the surface itself.

The solution sometimes crystallizes suddenly in the test-tube as though something had fallen in. The crystallized drops will not stand exposure to air for more than 30 minutes or so. Hence, although there is plenty of sulphuric acid in my laboratory, where I have often heated this solution, I find it very hard to believe that the salt exists in this form in any part of the room. The normal salt and the anhydrous salt are without action on the solution. It crystallizes in a test-tube in fine stellate masses, with projecting points on all sides, as alum sometimes does; these ultimately coalesce. These crystals are composed of very fine parallel fibres like ferns, and are opaque white. It sometimes sets in long fibres, radiating from different points like aluminium sulphate. Owing to the fineness of the fibres it would be very difficult to free them from the mother-liquor.

My reason for believing them to be a hydrate is this:—In a beaker this solution gradually deposits clear crystals, varying from very fine needles to rhombic plates, prisms, and short, nearly globular, highly modified forms. These are formed near the top, and may perhaps be different hydrates. They are formed, however, at the same time, and at present I believe them all to be the hyperacid salt. Similar ones are formed by putting the normal salt in the 6 to 1 solution, and this remains liquid, sometimes dissolving the crystals. An opaque amorphous mass is formed at the same time, which appears to be hydrated, but it also is inactive. A mixture of two parts of acid to one of salt in a flask, when boiled to get rid of all water, sets firmly in a clear mass, in which the opaque variety makes no change. Then if a little water is added the salt turns opaque white wherever the water reaches; and this is entirely absorbed, the cake remaining quite dry.

If this is again melted it deposits clear prisms, leaving a little mother-liquor; but the opaque variety when introduced from the 6 to 1 solution causes the whole mass to set firmly opaque white and become quite dry. The opacity spreads slowly, and a kind of beard of fine crystals can sometimes be seen growing round the prisms at the edge. Lovely foliated films are often formed at the

same time. The clear crystals are inactive in the 6 to 1 solution, while the opaque is active : and this is a clear proof of their identity. Solutions of intermediate strength between 2 to 1 and 6 to 1 often deposit in flasks the whole excess in clear crystals, which are sometimes inactive in the 6 to 1 in a test-tube. It is almost impossible to obtain these solutions supersaturated in flasks, though it may be done with the utmost facility in test-tubes. Out of many trials with one flask I only succeeded once by leaving it to cool on the sand-bath. In a test-tube they give the same forms as the 6 to 1. The variety of the forms in which these solutions crystallize is truly astonishing, according to the proportion of acid and salt, amount of water, and the temperature. A flask once gave the most exquisite little, flat, open flakes closely resembling snow-flakes ; but I have not been able to reproduce them. In short the relations of these two substances to each other want working out thoroughly. A certain amount of acid added to the salt which is in excess gives a thin liquid, which will not crystallize, and a little fine white powder, the anhydrous salt. Two drops of acid in a test-tube half-full of solution cause drops to evaporate on a plate in octahedra ; and when the anhydrous is thrown down on heating the test-tube locally after crystallizing, it is redissolved, leaving, however, well-marked octahedra just before it all disappears.

The most curious property, however, of the 6 to 1 solution is this :—On a clean glass plate it can be spread out into a thin covering of the plate with the handle of a tooth-brush ; then with the end of a glass rod scratch a letter hard on the plate, and the letter will come out at once in slowly growing crystals. The effect is certain with the right proportions, and is most striking, as a plate of any size can be used. Scratching has the same effect when the solution is placed on gold or copper, but not on platinum foil, lead foil, bone, gutta percha, or any soft substance. The effect is of course analogous to that of scratching on the ammonio-magnesic phosphate and on soda water in a clean tumbler. Mr. Tomlinson explains these by supposing that a partial vacuum is formed into which the salt and gas separate. I confess it seems to me more probable that the result is due to vibration. With the same solution of sodium sulphate in acid, but of different strength, scratching is inactive, and I have tried it in vain on many aqueous solutions.

I cannot see why the vacuum should not act equally on all ; but it is easy to understand how the molecular vibrations of one unstable system should be affected by a particular set of vibrations, whilst those of another system should not. The results obtained thus far, then, are :—

1. Exposure to air and dust has no effect on some supersaturated solutions.

2. The sulphates are the most sensitive. Exposure of a clean glass plate for half an hour to the air of my laboratory caused nearly all the drops of sodium sulphate put upon it to crystallize at once, whilst the same plate recently cleaned is quite inactive.

3. Even the sulphates are unaffected by the dust of the open air and generally of ordinary rooms.

4. Anhydrous salts or modified salts, sometimes new, are produced by the spontaneous evaporation of the solutions in drops.

5. Drops can be rapidly touched on their surface with crystals of the same salt without crystallizing.

6. Greasy surfaces, whether films or lenses, have no effect.

7. The shape of the vessel has sometimes a material influence on the possibility of obtaining a supersaturated solution.

8. Air and dirty surfaces are active on salts which apparently cannot exist in air.

9. Scratching a hard surface will cause a particular solution to crystallize.

The crystal theory, modified as it now must be, seems on the whole the best explanation of the phenomena. The case of the hyperacid sodium sulphate, however, remains to be explained. If the crystal theory is true, the order of sensitiveness of the solutions should be the order of comparative rarity of the salts; and this remains to be proved.

As to the cause of supersaturation, a good many facts seem to show that it is the anhydrous salt which enters into solution. The lower hydrates seem to be first formed, as in the case of sodium sulphate and the alums. In the case of the hyperacid sodium sulphate with two parts acid to one of salt, repeatedly boiled, it seems to be the anhydrous salt which is first deposited. When the aqueous solutions of sodium sulphate and the alums are made to crystallize, the modified salts become opaque white, while the hyperacid salt remains unchanged, and can be obtained unchanged by heating the opaque variety from the top so as to dissolve this, but not the anhydrous.

Against the theory that it is the anhydrous sodium sulphate in solution at low temperatures must be set the following fact.

Löwel, in his Tables of the solubility of the three forms of sodium sulphate, which are found in all our text-books, gives 412 parts of salt to 100 of water as the maximum solubility of the 10-atom salt; and this is the highest number for any of the three kinds. Now I have dissolved 600 parts of 10-atom salt in 100 of water at 37° C. without throwing down a trace of anhydrous.

I then warmed it: at 45° a doubtful trace of anhydrous; at 51° very few; at 60° still very few; at 67° about as much as would lie on a little-finger nail; at 75° eight or ten times as much, the liquid nearly opaque; at 80° a large quantity; boiled, the salt thickly covered the bottom of a large flask.

Now here the solution at 60° practically retained the whole of the 6 oz. of salt to 1 of water, while according to Löwel it should have retained only $2\frac{1}{2}$ oz.

Then between 70° and 80° a sudden change takes place, and a large quantity is thrown down. This agrees so far with Löwel's Table, as, according to him, at 84° the whole of the excess was practically thrown down. This looks very much like dissociation

taking place at that temperature; and that would involve the supposition that it was the 10-atom salt in solution before. The difference in our results springs from the different modes of working. Löwel always maintained a large excess of anhydrous present, whilst I added the salt in small portions, carefully avoiding throwing down any anhydrous. This is pretty easily done by keeping up a very rapid motion so as to prevent the liquid from getting heated too much at any point. It seems to me that in any case, as the six ounces fairly dissolved, the solubility of the 10-atom salt should be given in those proportions. Further experiments would, I have no doubt, give still higher figures.

In conclusion, I would remark that if the crystal theory of these solutions be accepted we have a test of great delicacy in these drops for the presence of the salts. Interesting experiments might be made as to the power of air to disseminate crystals of a salt thrown into it in fine powder.

De Coppet has already remarked that the mass of a solution exerts some influence on its crystallization, and I have shown that the form of the vessel also has a decided effect. The effect again of different vibrations on different solutions is worth trying, as there seems to be no reason why the hyperacid sodium salt should be an exceptional case.

A good deal of work has yet to be done before we arrive at a satisfactory explanation of these obscure phenomena.

GEOLOGICAL SOCIETY.

[Continued from p. 236.]

February 7th, 1877.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

2. "On new Species of *Belemnites* and *Salenia* from the Middle Tertiaries of South Australia." By Ralph Tate, Esq., F.G.S., Professor of Natural Science in the University of Adelaide.

3. "On *Mauisaurus Gardneri* (Seeley), an Elasmosaurian from the base of the Gault at Folkestone." By Harry Govier Seeley, Esq., F.L.S., F.G.S., Professor of Geography at King's College, London.

February 21st, 1877.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

1. "On possible Displacements of the Earth's Axis of Figure produced by elevations and depressions of her surface." By the Rev. J. F. Twisden, M.A., Professor of Mathematics in the Staff College.

The object of this paper is to discuss the question of the possibility of a displacement of the earth's axis of figure under the conditions indicated in a question (suggesting the possibility of a displacement of the axis of figure from the axis of rotation amounting to 15° or 20°) put to mathematicians in a passage of the Anniversary Address delivered to the Geological Society by its President, J.

Evans, Esq., on the 18th February, 1876. The treatment of the question is kinematical; the forces by which the elevations and depressions might be effected do not come under discussion. In determining numerically the amount of the deviation from the formulas investigated, approximate numbers seem to be sufficiently exact for every useful purpose. The conclusions arrived at are as follows:—

(1) The displacement of the earth's axis of figure from the axis of rotation that would be effected by the elevations and depressions suggested in the question above referred to would be less than 10′ of angle.

(2) A displacement of as much as 20° could be effected by the elevations and depressions of the kind suggested only if their heights and depths exceeded by many times the height of the highest mountains.

(3) Under no circumstances could a displacement of 20° be effected by a transfer of matter of less amount than about a sixth part of the whole equatorial bulge.

(4) Even if a transfer of this quantity of matter were to take place, it need not produce any effect, or only a small effect, on the position of the axis of figure; *e. g.* if it took place in a way resembling that suggested in the question, it would produce a displacement amounting to but a small part of 20°.

(5) If, however, we suppose a deviation of the axis of figure from the axis of rotation amounting to as much as 20° to have been by any means brought about, the effect would be to cause a sort of tidal motion in the ocean, the greatest height of which would tend to be about twice the depth of the ocean. The author suggests as probable that the effect of this tendency would be to cause the ocean to sweep over the continents in much the same way that a rising tide sweeps over a low bank on a level shore.

(6) The notion that a large deviation of the earth's axis of figure from its axis of revolution may be effected by elevations and accompanying depressions is at first sight an inviting way of bringing polar lands into lower latitudes, and thereby accounting for the more genial climate that is believed to have once prevailed in such countries as Greenland. The investigation by which the above results have been obtained seems to show that the desired explanation is not to be sought in the direction indicated by Mr. Evans's question. Whether there is any other agency by which a gradual displacement of the pole geographically could be effected is a question of far wider scope than that discussed in the present paper, and one which the author does not profess to determine *.

2. "Note on a Specimen of *Diploxylon*, from the Coal-formation of Nova Scotia." By J. W. Dawson, LL.D., F.R.S., F.G.S.

* The first draught of the paper, of which the above is an account, was drawn up last August, and was shortly after sent to Mr. Evans. It was written independently of the wider view of the subject taken by Sir W. Thomson in his Address delivered at the last Meeting of the British Association, and by Mr. G. Darwin in his paper, of which an abstract has been published in No. 175 of the Proceedings of the Royal Society.

March 7, 1877.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

1. " On the Vertebral Column and Pelvic Bones of *Pliosaurus Evansi* (Seeley), from the Oxford Clay of St. Neot's, in the Woodwardian Museum of the University of Cambridge." By Harry Govier Seeley, Esq., F.L.S., F.G.S., Professor of Geography in King's College, London.

2. " Supplementary Notes on the Fauna of the Cambridge Greensand." By A. J. Jukes-Browne, Esq., B.A., F.G.S.

3. " On the Beds between the Gault and Upper Chalk, near Folkestone." By F. G. Hilton Price, Esq., F.G.S.

The author described the characters presented by the beds between the Gault and Upper Chalk near Folkestone, indicated the fossils contained in them, and their range in this division of the Cretaceous series, and discussed the classification of the deposits, and their equivalence with those recognized by other writers. His conclusions are shown in the following tabular arrangement:—

C. Barrois.		Author's divisions and zones.	F. Drew in Whitaker's Survey Memoir.	D'Orbigny.
		Upper Chalk.		
Craie marneuse à *Ter. gracilis*, about 75 feet.	*Lower Chalk with Inoceramus labiatus.*	IX. Zone of *Echinoconus subrotundus* and *Terebratulina gracilis*, 118 feet.	Concretionary ? nodular chalk, 73 feet.	Turonian, 150 feet.
Craie noduleuse à *Inoc. labiatus*, 75 feet.		VIII. Zone of *Cardiaster pygmæus*, 32 feet.		
Bel.-plenus zone proper.		VII. Zone of *Belemnites plenus*, 4 feet.	Soft bed of 6 feet with Belemnites, Whitaker, p. 33.	
Zone à *Bel. plenus* (Craie compacte).	*Grey Chalk.*	VI. Zone of *Holaster subglobosus*, 148 feet.	White chalk without flints, 138 feet.	
Craie argileuse avec bancs durs à *Amm. rhotomagensis*.				
Zone à *Amm. varians*.		V. 2 ft. 9 in. IV. Zone of *Amm. rhotomagensis*. 11 feet. III. 8½ feet.		Cenomanian, 197 feet.
Craie marneuse à *Plocoscyphia mæandrina*.	*Chalk Marl.*	II. Zone of *Plocoscyphia mæandrina*. 10 feet. I. 14 feet.	Chalk marl, thickness not given; say about 30 feet.	
Marne sableuse = zone of *Pecten asper*, or Warminster beds.				
		Upper Gault.		

(Left vertical label: Craie à Holaster subglobosus.)

XLII. *Intelligence and Miscellaneous Articles.*

ON DIFFUSION AND THE QUESTION, IS GLASS IMPERVIOUS TO GASES?

BY G. QUINCKE.

IT is usual to attribute to all bodies the property of porosity. Respecting the magnitude of the pores or of the molecules of which the bodies consist we know as good as nothing. It might, however, well be possible that composite molecules, especially those with greater molecular weight, occupy a greater space, and constitute bodies with wider pores, than those whose molecular weight is less. A hydrogen molecule would then occupy the smallest space, and it would be conceivable that hydrogen particles might pass through the pores of solid bodies like glass.

Opposed as these views may appear to a now very prevalent hypothesis concerning the nature of gases, the question can only be decided by experiment. For this purpose, I have tried for years to force hydrogen and carbonic acid, by pressures of from 40 to 120 atmospheres, through a glass wall of 1·5 millim. thickness, and to determine, by the loss of weight, the quantity of gas that had passed through.

One leg of a V-shaped glass tube was a capillary tube of 200 millims. length, closed above; the other was a tube contracted in the middle and open above, 150 millims. long, 8 millims. in diameter, and its wall 1·5 millim. thick. Into the open leg a drop of quicksilver was put; upon this dilute sulphuric acid was poured; into the upper part some sheet zinc was pushed, which was kept from contact with the acid by the contraction of the tube; and then the open end was carefully closed by fusion at a glass-blower's lamp. Four tubes thus prepared underwent a double weighing; and then, by inclining the tubes, the sulphuric acid was brought into contact with the zinc.

The pressure of the hydrogen was shown by the diminution of volume of the air in the capillary tube, which served for a manometer. Its amount on the first day, in the different tubes, was from $1\frac{1}{2}$ to 10 atmospheres, rose in five months up to 27–54 atmospheres, and in 17 years up to 25–126 atmospheres. During this time the tubes were many times doubly weighed on an excellent balance; and exactly the same weight, within from 0·1 to 0·3 of a milligram, was always found (8·2556–16·5461 grams).

Another similar tube, with carbonate of lime and concentrated sulphuric acid, in which the pressure of the carbonic acid gas amounted on the first day to 21 atmospheres, after five months to 34 atmospheres, and after seventeen years to 44, showed likewise always the same weight (14·6361 grams).

Thus, according to these experiments, a pressure of from 40 to 100 atmospheres cannot, during a space of seventeen years, force through 1·5 millim. thickness of glass a perceptible quantity of carbonic acid.

While at the commencement the concentrated sulphuric acid wetted the glass sides of the tube, and showed a sharp marginal angle (apparently 0°), gradually in the course of years the angle

has become obtuse, and the acid flows in the tube with condensed carbonic acid like quicksilver in a glass tube filled with air.

In the atmosphere of hydrogen the angle at the margin of the dilute sulphuric acid, which at first likewise wetted the sides, has also increased to about 60°.

The glass thus appears to have gradually in the course of years, under the influence of the great pressure, become coated with a thin layer of carbonic acid or hydrogen respectively, which exerts a different attraction from that which glass exerts upon the liquid particles at the margin of the surface. A similar film of gas must have been deposited on the surface of the zinc and obstructed the further chemical action of the acid *.

In spite of the negative result of these experiments, I might not conclude that the molecules of hydrogen and carbonic acid have greater dimensions than the molecules or the pores of glass. The distance within which the molecular forces of glass act upon the gas particles is at all events greater than the dimensions of the molecules themselves. The pore-walls of the glass may be coated with a layer of absorbed gas which, through the vicinity of the solid substance, has itself become immovable, and hinders the passage of the gas particles from the interior of the tube into the outer air. It is also conceivable that there is in the pores of the glass a drop-forming liquid with strongly curved surfaces, which prevents the outflow of the gas, like as under ordinary conditions mercury does not flow out of the pores of a wooden vessel containing it.

A similar objection may be raised against M. Traube's* otherwise ingenious method of determining the relative magnitude of the molecules of a substance from the possibility or not of its passing through a so-called " precipitate-membrame." He brings together two substances soluble in water, A and B, which at their surface of contact give an insoluble precipitate. This precipitate forms a thin porous skin or a network. The meshes are smaller, M. Traube thinks, than the smallest particles of one of the substances (say A) which have contributed to form the precipitate ; for if they were larger, molecules of the substance A would go through them to the substance B and stop up the apertures with newly formed insoluble precipitate. According to this, the thin skin of " precipitate-membrane " represents a sieve, through which only molecules smaller than its interstices, or smaller than the molecules of the substance A, can pass ; substances with larger molecules cannot diffuse through this sieve or precipitate-membrane.

But herein the fact is lost sight of that the solid formed by the chemical action of the substances A and B will in general, by selective adsorption, hold different quantities of the three substances A, B, and water at its surface. By the thickness of this

* Compare Babinet, *Ann. de Chim.* (2) t. xxxvii. p. 183; Faraday, Quart. Journ. iii. p. 374; Gmelin, *Handb. d. Chem.* i. p. 126 (1843); L. Meyer, Pogg. *Ann.* vol. civ. p. 189 (1858).

† Reichert und Dubois-Reymond's *Archiv,* 1867, p. 87 *seqq.,* "Experiments for the Theory of Cell-formation and Endosmosis."

adsorbed film of liquid, which has become almost solid, the magnitude of the pores of the " precipitate-membrane " through which the diffusion takes place is again determined ; while the thickness of the adsorbed film may depend upon the velocity with which the insoluble substance of the membrane has been produced.

This theory of diffusion through sieve-like precipitate-membranes is especially supported by experiments with a watery solution of tannic acid and so-called β gelatine—that is, ordinary concentrated gelatine solution which has been kept for a long time heated to 100° C. and has thereby acquired the property of remaining liquid even after cooling. A drop of this solution, cooled upon a glass rod, brought into a solution of tannic acid, forms, in the course of an hour or two, a little sac filled with fluid, which gradually grows larger until finally it bursts or again shrivels up. M. Traube thinks that here a precipitate-membrane of insoluble tannate of gelatine is formed, through the pores of which no tannic acid, but only water, can come from the outside to the gelatine solution in the interior.

I have left for twenty-four hours a drop of warm β gelatine to dry adhering to the lower aperture of a short, light funnel blown from a clean glass tube, and then placed the funnel as a float on a five per cent. solution of tannic acid. After three hours a sac, filled with fluid, had formed out of the gelatine, which was bounded by air in the interior of the funnel, and was penetrated by a glass thread introduced into the upper opening of the funnel without injuring the bottom part adjacent to the tannic acid. A portion of the fluid ascended out of the interior of the sac, by the capillary attraction of the glass thread ; and, emptied into a watch-glass, it gave, with chloride of iron, a deep-black colouring. It appears that the gelatine sac contained in its interior at first much, but after a longer diffusion a less quantity of tannic acid. Thus, in contradiction to the theory of the sieve-like precipitate-membranes, tannic acid penetrated from without into the interior of the precipitate-membrane covering the gelatine ; and the process of the diffusion of gelatine and tannic-acid solution is much more complicated than it is conceived to be by that theory.—Poggendorff's *Annalen*, 1877, No. 1, vol. clx. pp. 118–123.

ON COSMIC VULCANISM.   BY M. TSCHERMAK.

The present is in continuation of a previous paper, on the probable mode of formation of meteorites, in which the opinion was expressed that, judging according to our present knowledge, all stellar systems in their development pass through a volcanic phase.

The crater-form of the lunar mountains, the eruptive phenomena in the sun, the change of brightness of stars, the nature of meteorites (which for the most part resemble volcanic tufas) are all facts which, it may be conjectured, are connected by a common bond. But the attempt to bring these perceptions under one point of view with our experience of terrestrial volcanoes fails, if based on those hypotheses which have recently come to the front.

One of these hypotheses, which derives the vulcanism of the earth merely from water penetrating into the glowing depths, is unsuitable for such a generalization, because both the phenomena in the sun and the absence of water on the moon are incompatible with its presuppositions. A second hypothesis, which takes for its principle the conversion of work into heat, and, in accordance with Mallet's experiments, assumes that volcanic phenomena are conditioned by the heat arising from the sinking by contraction of the earth's crust, meets with a not unfounded opposition from many quarters, since the quantity of heat on which this view rests is so insignificant that, according to the calculation of the propounder himself, it could produce in the most favourable case a rise of temperature from 15° to 55° C.

The assumption made use of by Nasmyth and Carpenter in order to account for the eruptive formation of the lunar craters, when they referred the former volcanic activity of the moon to contraction of volume in solidifying, is also improbable in itself and incapable of general application.

On the contrary, an older hypothesis, which has hitherto met with but little notice, is of high importance for the explanation of cosmic vulcanism. It assumes that the volcanic phenomena of the earth are effected by gases and vapours which are contained absorbed in the supposed fluid interior of the earth, and are evolved as it gradually solidifies. It is true that, as remarked by Angelot, who occupied himself with the consideration of this idea, it is not sufficient for the complete explanation of the earth's vulcanism; but it completes the explanation which is based upon the penetration of water into the depths in the most important points, especially in its chemical aspect; and, besides, it admits of application to the other heavenly bodies, in that it represents their eruptive phenomena as a consequence of progressive cooling.

This view has moreover the advantage over its competitors, that it is already contained in that more general hypothesis set up by Kant and Laplace for the purpose of making intelligible the formation of the solar system. If the production of the heavenly bodies be conceived as an aggregation into spheres of such materials as are represented in the earth, it must be admitted that the formed globes of glowing liquid contain absorbed matters which may be evolved from them in a gaseous form and occasion eruptions.

Observations on many glowing liquid bodies, such as volcanic lavas, cast iron, melted copper and silver, &c., show that, especially under a higher pressure, they are capable of absorbing large amounts of gaseous substances, and giving them out again on solidifying. Accordingly those materials which, according to modern views, are imagined in the interior of the earth and in the neighbouring heavenly bodies, are of such a nature as to evolve masses of gas on cooling.

The application of the above to the sun is self-evident. Meteors are derived from very small stars, which in their rapid cooling fall into eruption, and therewith are in part or wholly disintegrated. The surface-features of the moon can in like manner be traced

back to a volcanic stage conditioned by its cooling; and the absence of an atmosphere can be explained by the nature of the materials which, judging from the small specific gravity of that heavenly body, compose its crust and are capable of confining the volcanic vapours.—*Sitzungsb. der Wiener Akademie, math.-naturw. Classe,* 1877, No. vii. pp. 62–64.

RESEARCHES ON HEAT-SPECTRA.   BY P. DESAINS.

In a research published in May 1870 I established that, in the solar spectrum formed by a rock-salt apparatus, the heat accompanying the luminous rays is about one third of the total heat; on the contrary, in the spectrum of incandescent platinum it is only an insignificant fraction of it.   The results are similar when refracting prisms entirely of flint glass are employed.   I tried in vain to cause this difference to vanish by transmitting the radiations from the incandescent metal through more or less thick layers of water.   In my experiments the dark portion of the spectrum of platinum had an extent of about 4°; the interposition of a layer of water of 1 centim. thickness reduced to 2° the length of this obscure spectrum, and diminished its intensity to nearly three fourths of its primitive value; but the luminous heat still remained a very small fraction of the total heat.   Greater thicknesses of water shortened the dark region so as to leave it only a much less extent than in the solar spectrum.

Spectra obtained with the electric lamp, on the contrary, may be rendered in their totality much more like those obtained with the rays of the sun.

In the electric spectrum, at first, heat is found as far as into the blue.   De la Provostaye and I verified this fact more than twenty-five years since; and in some recent experiments, the heat in the luminous part of a like spectrum appeared to me to be about one sixth part of the total heat.   It is true this ratio is only half of that found in operating with the solar rays; but if we pass the radiation of the electric lamp through from 3 to 4 centims. of water, we notably reduce the calorific intensity of the dark portion of the spectrum, almost without modifying the luminous heat; and this latter then becomes about one third of the total, just as it does in the solar spectrum.

The spectrum thus obtained is nevertheless not absolutely identical with the solar spectrum; in particular, it has less extent than the latter, especially at the violet end; but the curves representing the intensities in the two spectra exhibit only very slight differences from one another throughout the region comprised between the middle of the green and the portion of the dark spectrum symmetrical with the blue; and this is especially the calorifically effective region.   I will add, in conclusion, that the pile employed in the experiments here described consisted usually of 50 large Bunsen elements; sometimes, however, the number of these elements reached 100.   Finally, it will not be useless to call to mind that, according to the usual estimates, the vapour contained in an atmospheric column extending vertically to the limit of the

atmosphere would form, after condensation, a stratum of water the thickness of which would differ but little from 4 centims.—*Comptes Rendus de l'Académie des Sciences*, Feb. 12, 1877, tome lxxxiv. pp. 285, 286.

ON THE POLARIZED LIGHT OF THE RAINBOW.
BY PROF. J. DECHANT.

In vol. clix. of Poggendorff's *Annalen*, M. Schiel communicates the observation that the light of the rainbow is completely polarized. This fact had also previously been observed by Tyndall, in 1870, on the occasion of his journey to Algeria*; and he also states the direction of the polarization, saying, " The light of the bow could in all cases be extinguished by a Nicol's prism the greater diagonal of which was placed tangential to the arc."

The phenomenon finds its satisfactory explanation in the reflection of the light at the back of a rain drop. For, in the first place, the direction of undulation of the light which arrives at the eye from the rainbow agrees with that of the polarized light produced by reflection. If, namely, the light of the rainbow is extinguished by the Nicol with its longer diagonal held parallel to the tangent to the bow, the undulations of the light itself take place in the direction of the longer diagonal, consequently parallel to the tangent. But parallel to the tangent is perpendicular to the plane which passes through the sun, the rain drop, and the eye; hence it follows that the undulations are perpendicular to the plane of incidence.

Secondly, however, the angle under which the rays fall upon the posterior wall of the drop is not far from the angle of complete polarization ; for while the latter for water in air amounts to about 37°, the former averages 40°.

Calculating according to Fresnel's intensity-formula (and taking into account the double refraction in the rain drop) the ratio of the intensities of the light whose undulations are perpendicular to, and of that whose undulations are in, the plane of incidence, we get

$$\left[\frac{\cos^3{(\alpha - \beta)}}{\cos{(\alpha + \beta)}}\right]^2,$$

where α denotes the angle of incidence of the effective rays, and β the angle of refraction. This gives for the extreme red rays ($n = 1.3317$) 24·5, and for the extreme violet ($n = 1.3448$) 34·9.

Thus the rainbow appears from 25- to 35-fold fainter when the Nicol is rotated 90° from the position in which the shorter diagonal is held parallel to the tangent of the bow—which certainly comes very near complete extinction when we consider that the secondary bow already appears tolerably faint whose intensity according to the calculation is yet only $2\frac{1}{2}$-fold less than that of the primary.

Lastly, we can also indirectly, by an experiment, convince ourselves of the correctness of the explanation above given, if we try to obtain a rainbow with a liquid in which light is not so strongly polarized by reflection at the back of a drop as in water. For this purpose we first seek the exponent of refraction of the liquid with which light would be completely polarized.

* *In den Alpen,* deutsche Ausgabe, 1872, S. 382.

If β is the angle under which the rays fall upon the back of the drop, and n the refraction-exponent (from air into the liquid), then

$$\tan \beta = \tfrac{1}{2} \sqrt{\frac{4-n^2}{n^2-1}}.$$

But $\tan \bar{\ } \beta$ must be also equal to $\dfrac{1}{n}$ if complete polarization is to enter; from this we find

$$n = \sqrt{2} = 1 \cdot 414 \ldots.$$

We must now select a liquid whose refraction-exponent is as distant as possible from $\sqrt{2}$, consequently oil of cassia for instance, or sulphide of carbon—of which the first is more suitable for producing a rainbow, since, not to mention that its smell is less unpleasant, it more readily scatters in fine drops, which remain longer floating in the air. Calculation gives as the intensity-ratio of the light whose undulations take place perpendicular to, and of that whose undulations are in, the plane of incidence, for the red rays ($n = 1 \cdot 5945$) 6·3, and for the violet rays ($n = 1 \cdot 7025$) 2·8.

In fact, when oil of cassia is scattered in sunlight before a dark background by a current of air driven through a fine glass tube at right angles above another tube which dips in the liquid, a splendid rainbow is seen, the light of which cannot at any intensity be completely extinguished by the Nicol.—Poggendorff's *Annalen*, 1877, No. 1, vol. clx. pp. 123–125.

ON THE NATURE OF GAS MOLECULES.
BY LUDWIG BOLTZMANN, OF GRAZ.

Since the assumption that gas molecules behave as aggregates of material points (atoms) led to results not in accordance with experience, it has been dropped by the author, and the hypothesis is adopted that we are permitted, in calculating the push-action of the molecules, to regard as approximately rigid the aggregate which we designate as a single gas molecule, and which may consist of various corporeal and probably also æthereal atoms. He finds, on the basis of his earlier results generalized by Maxwell and Watson, that then the ratio of the heat-capacities of a gas must be $1\frac{2}{3}$ when its molecules have a spherical form. The ratio of the heat-capacities becomes equal to 1·4 if the molecules have the form of rigid solids of rotation which are not spheres, and $1\frac{1}{3}$ if they are rigid bodies of any other form whatever. These numbers appear to accord at least so far with those found by experiment, that it cannot be said that experiment furnishes any confutation of the theory thus modified. It is also pointed out that the values found experimentally for the heat-capacity of gases on this hypothesis are in satisfactory accordance with the heat-capacities of solids. It is self-evident that gas molecules cannot be absolutely rigid bodies; this is disproved by spectrum-analysis. It may be that the vibrations which give rise to gas-spectra are only brief agitations lasting during the collision of two molecules, comparable to the sound-exciting vibrations which ensue when two ivory balls strike one another.—Poggendorff's *Annalen*, 1877, No. 1, vol. clx. p. 175–6.

XLIII. *On Rotation of the Plane of Polarization by Reflection from the Pole of a Magnet.* By JOHN KERR, *LL.D., Mathematical Lecturer of the Free Church Training College, Glasgow*[*].

1. I WAS led some time ago to think it very likely, that if a beam of plane-polarized light were reflected under proper conditions from the surface of intensely magnetized iron, it would have its plane of polarization turned through a sensible angle in the process or fact of reflection. The known facts upon which this expectation was founded are indicated briefly under the five following heads.

(1) The effects discovered by Faraday in his famous polariscopic experiments in the magnetic field.

(2) Many instances in optics to this effect—that a reflected vibration may have its character determined wholly or partly by the refractive power of the reflector, or, more generally, by the specific properties of the reflecting body in relation to transmitted light. I may adduce Brewster's law of the polarizing angle, also Fresnel's theory of reflection from glass &c., a theory which is still accepted and applied in delicate photometric work as affording a good expression of facts, and which treats refraction and reflection as closely related parts of one dynamic whole. I may adduce also the laws of reflection from the surfaces of Iceland spar and other birefringent bodies. It is true that in this last case the catoptric effects are extremely faint in comparison with the dioptric—a fact which is clearly unfavourable to the proposed case of reflection from iron, as contrasted with the resolved cases of transmission through

[*] Communicated by the Author.

heavy glass &c.   But I think that the following facts bear with equal or greater force the contrary way.

(3) The enormous differences (in relation to magnetic force) between iron and steel on the one side, and Faraday's transparent diamagnetics on the other.

(4) The effects obtained by Verdet in his application of Faraday's magneto-optic method to the salts of iron.   The strongest instance is that of the perchloride.   A dense solution of perchloride of iron in wood-spirit gives a rotation of light contrary to, and nearly twice as great as, that given by heavy glass under the same conditions.

(5) The known laws of metallic reflection, particularly the fact that silver, zinc, steel, and other metals are distinguished from each other in a perfectly definite manner as reflectors, each metal having specific relations to the principal component vibrations (perpendicular and parallel to the plane of incidence) with reference both to change of phase and change of amplitude.

The preceding facts were sufficient to suggest a plan of procedure, as well as to give me a strong expectation of success.

During the month of August last, in the course of some careful experiments in the direction thus indicated, I obtained several interesting results which appeared conclusive.   Soon afterwards I gave a description of the experiments before the British Association.   Since that time I have made one or two additional observations, and have got rid of a serious error into which I had fallen in my first view of the facts.   In this paper I propose to give an account of all my principal experiments and views upon the subject.   And first, for future reference, I shall lay down the sum of the results in one sentence.

The new Fact.

2. When plane-polarized light is reflected regularly from either pole of an electromagnet of iron, the plane of polarization is turned through a sensible angle in a direction contrary to the nominal direction of the magnetizing current; so that a true south pole of polished iron, acting as a reflector, turns the plane of polarization righthandedly.

Apparatus and Arrangements.

3. *The Magnet.*—This is an upright horseshoe electromagnet, and a very good instrument, I think, of its size.   Only one limb of the horseshoe is used at a time, the current being sent through one of the coils, and the observations being made on the enclosed core.   Each of the cores is a solid cylindrical bolt of soft iron, 10 inches long and 2 inches in diameter, which is therefore the diameter of each polar surface.   Each

of the coils weighs 14 pounds; and the wire makes about 400 turns. The particular coil employed in any case is put into circuit (generally as a double wire of 200 turns) with a small Grove's battery of only six cells; and this is the highest power applied in my experiments. In the circuit is placed also a commutator, which is at my hand, so that, while I watch the polariscope, I have the magnetic state of the core under perfect control.

4. *Polar surfaces.*—These were originally well planed, and perpendicular to the axes of the cores. For the present purpose they had to be smoothed and brightened by polishing, a process which I found troublesome and excessively tedious, from the refractory nature of the material. The polishing was done with fine dry emery powder, applied by chamois leather to one of the surfaces, and by a rubber of fine silk stuff to the other. Each rubber was backed by a flat and smooth block of iron, which was worked carefully by hand over the end of the core. The last stage of the polishing was similar to the earlier stages, but without new additions of emery. When the process was finished, each polar surface (though not such a speculum as would satisfy an optician) acted as a pretty good plane metallic mirror, its plane perpendicular to the axis of the core. Placed in a room in ordinary daylight, each mirror gave good regular images of all surrounding objects that were in any degree illuminated; and in a darkened room, the image of a neighbouring candle-flame was generally very good, brilliant, distinct, and sharply and truly outlined, except towards the rim of the mirror, parts not used in the observations. The surface that had been finished by chamois leather was rather more brilliant than the other, but not so perfectly well planed.

I should say here that, from all that I have seen in these experiments and in some earlier trials, I consider the finest attainable polish very desirable. In my present apparatus, I would prefer a much finer polish to any increase whatever of magnetic power (3).

5. *Placing of the pieces.*—The electromagnet is placed on a solid table, near the edge, and is inclined with its polar surface towards the light by means of a small block placed under the stand. The source of light is a paraffin-flame, narrow and very brilliant, distant a foot or less from the polar surface. Close to the flame stands the first Nicol. The beam of plane-polarized light so rendered is incident horizontally (at an angle of 60° to 80° to the normal) on the polar surface, and is regularly reflected. On this side of the polar surface, a few inches distant, comes the second Nicol, which is supported on a lateral

Y 2

stand, and so placed that, when I look fairly through it, I see the image of the flame in the iron mirror.

6. *Principal azimuths of first Nicol.*—As the polariscope is worked here in the usual way, by restoration from the best possible extinction, there are only two positions of the first Nicol which are suitable to start from. The plane of polarization of the light incident upon the iron mirror must be either parallel to the plane of incidence or perpendicular to it, because in every other case the reflected light is elliptically polarized and therefore inextinguishable by the analyzer. I generally make the plane of polarization coincide with the plane of incidence; and I manage this in the first place very approximately by trial. I lay the first Nicol with its principal section sensibly horizontal. Looking through the second Nicol, and watching the image of the flame in the polar mirror, I turn the second Nicol quickly through the position of minimum intensity backwards and forwards, while the first Nicol is turned slowly, also backwards and forwards, until I obtain a minimum-intensity zero.

It is a matter of capital importance in the experiments to have the Nicols placed in this position of pure extinction; and the arrangement is not so easily made as might be supposed. Perhaps it is from imperfection of polish, and perhaps from the very nature and structure of the reflecting metal; but whatever be the reason, the mirror is never perfectly black in the polariscope; and though the intensity of the illumination is very faint when the Nicols are in exact position, it is still sufficient to embarrass the observer's judgment when he has to decide between pure extinction and impure. The difficulty can be overcome by a simple and regular process, as will be seen immediately. In the mean time I assume that we can obtain a pure initial extinction in the polariscope.

7. *Submagnet.*—I have now mentioned every thing that is of any importance in the arrangements, except one condition, without which I have never obtained any optical effect; and that is, an intense concentration of magnetic force upon the iron mirror. For this purpose I employ a block of soft iron, one of several polar pieces belonging to the magnet, 2 inches square and 3 inches long, which has been planed off at one end into a blunt wedge with well-rounded edge. Two splinters of hard wood, which have been thinned and toughened by hammering, are laid upon the sloping polar surface about an inch apart, and parallel to the plane of incidence. Holding the wedge in my left hand, I plant it edge downwards upon the splinters, with its rounded edge perpendicular to the plane of incidence, and right above the centre of the mirror. The

effect of this arrangement is, that when the circuit of the magnetizing current is closed, there is a very powerful concentration of magnetic force upon the mirror, and particularly on that part of it which is utilized optically in the experiments, on so much of it, namely, as the chink between wedge and core leaves exposed, on one side to the lamp, and on the other side to the observer's eye. The lines of magnetic force are sensibly perpendicular to the reflecting surface. The iron mirror is a true polar surface; and its intensely contrasted states, as north, south, neutral, are perfectly under control through the commutator.

The wedge intercepts a large part of the image of the flame. The pieces are generally so placed that the part left of the image is a strong middle segment, both top and bottom being cut off. The object now watched in the polariscope is a broad streak of light, crossing the chink at right angles from top to bottom, very sharply defined, and perfectly suitable as an object in delicate polariscopic work.

Splinters of different thicknesses are employed in different experiments, and in variations of one experiment. My only rule is, that the chink between block and core be as narrow as the requirements of the optical observation will allow. On an average, the width of the chink in the following experiments is about $\frac{1}{20}$ of an inch. The arrangements now described (3 .. 7) are shown simply in the adjacent diagram.
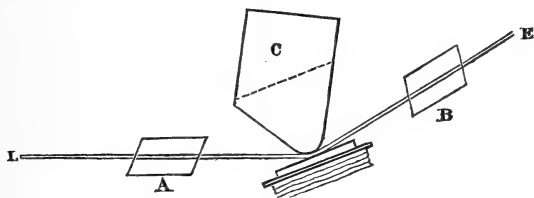
L is the source of light, E the observer's eye, A and B the first and second Nicols, C the wedge of soft iron.

8. *First experiment.*—The pieces arranged as in the diagram, the chink between block and mirror as narrow as possible, the plane of polarization of the light incident on the polar mirror parallel or perpendicular to the plane of incidence, and the second Nicol turned into the position of pure extinction. The observer now watches the chink through the second Nicol, and works the commutator. When the circuit is closed, the streak of light immediately reappears. The effect is very faint at the best; but it is very distinct and perfectly regular, unless the apparatus is in some way out of order, the mirror dimmed, or the battery working below its average power. Under ordinarily

good conditions, at the instant when the circuit is closed, the light shows itself faintly in proper form, size, and position across the formerly uniform chink, and so continues without sensible change as long as the current passes.   Break, and the light immediately disappears.   Reverse, and the light again appears and continues till the instant of break, when it disappears at once.

The beam reflected by the mirror of magnetized iron is certainly not plane-polarized, as is the incident beam (and the reflected beam also before magnetization) ; for when the light is restored by magnetic force from pure extinction as above, it cannot be extinguished by any rotation of the second Nicol in either direction ; nor (as far as I can judge of these faint effects and with the present means) is the light sensibly weakened by any such rotation.   The analyzer's position of extinction before magnetization is also (exactly or nearly) the position of minimum intensity after magnetization.

In many repetitions of this experiment, the angle of incidence varied from 60° to 80°, and was generally about 75°.

9. At this point I must ask the reader's attention to several terms; and to the sense in which I shall use them.   By a rotation of the first Nicol to the right, I mean a rotation which is right-handed (like the motions of the hands of a watch which faces the observer) when viewed from the point of incidence on the iron mirror.   By the north pole of a magnet, I mean " that which points, on the whole, from the north, and, in northern latitudes, upwards " *.

10. *Second experiment.*—Taking this experiment as a continuation of the first, and providing for the best effects, I suppose all the arrangements as before: I suppose also that make, break, and reverse of the commutator give bright, black, bright in the polariscope distinctly, however faintly.

(1) Leaving the circuit open and every thing else untouched, I simply turn the first Nicol ever so little to the right.   The amount of the rotation is important.   I have said it was ever so little ; and this generally gives effects distinct enough.   But when working for the best results, I determine the displacement of the first Nicol by this condition, that the intensity of the light restored in the polariscope by the displacement be

* Sir W. Thomson's papers on Electrostatics and Magnetism, § 445. It will be seen from the quotation that this is no innovation of mine. Having had this nomenclature brought to my attention recently by Sir William Thomson and very strongly recommended by him, I made it a matter of careful consideration and have determined to adopt it.   Like poles of the great Earth-magnet and of our artificial magnets ought to be similarly named ; and the northern pole of the Earth-magnet cannot with any propriety be called a south pole.

sensibly equal to that of the light restored formerly by mag-
netization in the first experiment. This being done, I watch
the faint light in the polariscope, and work the commutator as
formerly. But I must now specify the magnetic states of the
mirror.

When the mirror becomes a north pole, the light flashes up
at once to a sensibly higher intensity, which is sustained with-
out change as long as the current passes. When the circuit is
broken and the mirror demagnetized, the light falls at once
from the higher intensity to the primitive faint intensity, and
so continues as long as the circuit is open. When the mirror
becomes a south pole, the light falls from the primitive faint
intensity, down either to perfect extinction or extremely near
it. In favourable cases of this kind (that is, in cases properly
managed and in a well-darkened room) it is very striking to
look at the chink through the analyzer, searching in vain for
the faintest trace of the streak of light, and remembering the
displacement of the first Nicol. When the circuit is finally
broken, the light reappears at once as at first.

(2) Leaving the circuit open and everything else untouched,
I watch the faint light in the polariscope, and turn the first
Nicol backwards to the left, into the position of extinction and
a little beyond it, regulating the amount of rotation by the in-
tensity of the restored light as in the first case. I now watch
the light through the analyzer and work the commutator. It
would be superfluous to describe the magnetic changes of the
iron mirror, and the corresponding changes in the polariscope ;
the description would be word for word as before, with one
essential alteration. It is the south pole that now strengthens
the light, and the north pole that extinguishes or weakens it.

This experiment is much more easily managed than the first.
Let a good sensible extinction of the streak across the chink
be obtained by optical trial in the manner already described
(6), the plane of polarization of the incident light being either
parallel or perpendicular to the plane of incidence ; and let the
first Nicol be turned to the right, so far only as to render the
extinction sensibly impure. When the three states of the
mirror (north, neutral, south) are now made to succeed one
another rapidly, the contrast of bright, faint, dark in the po-
lariscope comes out in almost every case very distinctly.

Very often I have seen the second experiment give clear
effects as now described, in cases where, through partial ex-
haustion of the battery, the first experiment gave no sure effect
whatever.

11. I have given these two experiments as a simple and
exhaustive summary of a large number of observations which

were at first very perplexing, so irregular and apparently inconsistent were the phenomena. The chief cause of my perplexity I found afterwards to be a very interesting thing; and that was what I may truly call the exquisite delicacy of the magnetic mirror as a test for fixing the position of the plane of polarization of the incident light. One or two simple notes of actual observations will illustrate this point more distinctly than any general statement could do.

Things often happened thus. Working as in the first experiment and with ordinary caution, I started from good extinction, and found the north pole restoring the light, and the south pole much the same as open circuit. Trying to obtain better initial conditions if possible, I threw the two Nicols well out of position, and worked them carefully back to good extinction; and now, without any other observable change in the conditions, I found things reversed, the south pole clearly restoring the light, and the north pole much the same as open circuit. Here the magnetic mirror simply detected the impurity of the initial extinction, and characterized it, by strong contrasts of intensity in the polariscope, as due to a slight misplacement of the first Nicol (otherwise barely or not at all detectable), to the right in the first case, and to the left in the second.

Working sometimes with one of the mirrors (that which had been polished by chamois leather, and which was not so well planed as the other), at a particular part of its surface, and at large angles of incidence, I found the upper end of the streak clearly restored by the north pole and the lower end not, while the lower end of the streak was clearly restored by the south pole and the upper end not. There can be no doubt that in this case the magnetic mirror detected a slight difference of slope at those parts of the mirror which reflected the upper and lower ends of the streak. Say that the one part sloped a little downwards to the left, and the other a little downwards to the right; then the planes of incidence at the two places would be out of coincidence with the plane of polarization of the incident light, to the left in the first case, and to the right in the second.

Similarly, I have sometimes seen the right side of the streak restored by the north pole and the left side not, while the left side was restored by the south pole and the right side not. Irregularities and inconsistencies of this kind were explained perfectly by the second experiment as soon as it was discovered.

Finally, I observe here that the arrangements for the first experiment are best obtained through those for the second; and this is a point of some practical importance. Arranging

the apparatus as for the first experiment and with the greatest care, I find the effects of the two magnetizations unequal in almost every case.   Say that the north pole restores distinctly, and the south pole weakly or not at all.   Leaving the circuit open, I turn the first Nicol to the left as little as possible, and then bring the second Nicol into the position of extinction, and test by working the commutator and watching the light in the polariscope.   Several careful operations of this kind are sometimes requisite.

Summary and Interpretation of the facts.

12. In these experiments light is reflected from an iron mirror at an incidence of 60° to 80°, passing through a first Nicol before reflection and through a second Nicol after.

Initial conditions.—The iron mirror unmagnetized, the principal sections of the two Nicols perpendicular and parallel respectively to the plane of incidence.

Essential operations.—Starting thus from pure extinction in the polariscope, we apply any one or two of four operations. Two of these are merely mechanical, extremely small rotations of the first Nicol from its initial position, a right-handed rotation (R) and a left-handed (L).   The other two are physical, intense magnetizations of the mirror, as a north pole (N) and as a south pole (S).   These four operations will be named here and afterwards by suggestive and easily remembered letters as above; and they will be grouped in pairs invariably, R and N together, thus:

$$(R, N),   (L, S).$$

13. When any one of the operations is applied singly, the light is restored from pure initial extinction in the polariscope.

When any two of the operations are applied simultaneously, and their relations determined by comparison of effects in the polariscope, they are found to be conspiring operations if they belong to the same pair, and contrary operations if they belong to different pairs.   Considering, then, any one of the operations, we see that there are two ways of strengthening its effect in the polariscope, and two ways of weakening it.   To strengthen the effect of R, apply either operation of the pair (R, N); turn the first Nicol a little more to the right, or magnetize the mirror as a north pole.   To neutralize or weaken the effect of R, apply either operation of the pair (L, S); turn the first Nicol a little to the left, or magnetize the mirror as a south pole.

To obtain a complete interpretation of the facts, we have

only to assume that the immediate optical effects of the four operations (R, N), (L, S) are similar in kind for all, and similarly directed for those of either pair, but oppositely directed for different pairs.   R and L turn the plane of polarization ; so therefore, according to this view, do N and S.   R and N turn the plane of polarization in one direction ; L and S turn it in the contrary direction.   But even from an optical point of view there is still an important difference between the mechanical operations and the physical ; for in the one case (R or L) the full effect of the operation is impressed upon the light before incidence, while in the other case (N or S) the effect is impressed somewhere and somehow in the very process of reflection.

To get a more definite statement of this interpretation, consider the pair of conspiring operations (R, N).   In the case of operation N, and to an eye which looks into the polar mirror, the nominal direction of the magnetizing current round the core is right-handed (9).   In the case of operation R and to the same eye, the direction of rotation of the plane of polarization, or the direction of rotation of the trace of that plane upon the reflecting surface, is evidently left-handed (9).   We infer that a right-handed current gives a left-handed rotation of the plane of polarization.   And this completes the first experimental proof of the general statement made in art. 2.

14. To test the truth of this view of the facts, I thought of three methods which appeared accessible :—first, to apply each of the four operations (R, N), (L, S), and to characterize them separately by definite compensating actions in the polariscope ; secondly, to apply the operations N and S in combination with small permanent rotations of the second Nicol ; thirdly, to return to the case of perpendicular incidence, which I had already tried roughly without success.   I shall prepare the way for an account of the first of these methods by a short mathematical discussion.

Compensation of effects of operation R.

15. Let the angle of incidence be about 75° ; and suppose that the initial conditions are as in the first and second experiments (12), and particularly that the direction OX of the vibration is perpendicular to the plane of incidence.   The operation R being now applied, and the incident vibration being turned thus through a small angle $XOC=\alpha$, it is required to find the character of the reflected light, particularly with a view to compensation.   The two rectangular components (one in OX) of the incident vibration (c in OC) are

$$c \cos \alpha \cos 2\pi \frac{t}{\tau},$$

$$c \sin \alpha \cos 2\pi \frac{t}{\tau},$$

or, more briefly,

$$a \cos \theta \quad \text{and} \quad a' \cos \theta,$$

where θ is proportional to t. Let O Y be perpendicular to O X and to the reflected ray; then, to obtain the components x and y of the reflected vibration in the directions O X and O Y, we must apply to the preceding components the known laws of metallic reflection. We find thus

$$\left.\begin{array}{l} x = ha \cos \theta, \\ y = ka' \cos (\theta - \phi), \end{array}\right\} \quad \cdots \quad \cdots \quad (1)$$

where h and k are constants characteristic of the reflecting metal. As the angle of incidence is about 75°, and therefore very near the principal incidence, we may put

$$\phi = \tfrac{3}{2}\pi. \quad \cdots \quad \cdots \quad \cdots \quad (2)$$

Substituting in (1), and representing the amplitudes by b and b', we find

$$\left.\begin{array}{l} x = b \cos \theta, \\ y = -b' \sin \theta. \end{array}\right\} \quad \cdots \quad \cdots \quad (3)$$

From these equations or otherwise we see that the reflected vibration is elliptic, and that its principal rectangular components are perpendicular and parallel respectively to the plane of incidence. We see also that the elliptic polarization is left-handed in the case of operation R, and right-handed in the case of L.

Hence a simple method of compensating the effect of the operation R, or of the rotation α of the incident vibration. Introduce a difference of phase $\frac{\pi}{2}$ between the components x and y by means of a quarter-wave plate, and then turn the second Nicol in the proper direction through a small angle which is definitely related to α. This method I have not had an opportunity of trying.

To find another method. Let the elliptic vibration (3) be represented by its rectangular components x' and y', in direc-

tions O X′ and O Y′ inclined at 45° to O X; and let
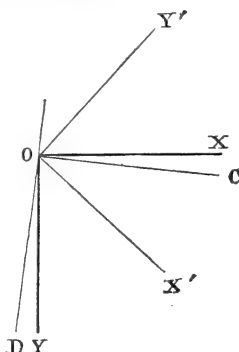
$$x' = m \cos (\theta - \beta),$$
$$y' = m \cos (\theta - \gamma).$$

Identifying the second members of these equations with the proper sums of resolved parts of x and y, we find easily

$$m^2 = \tfrac{1}{2} (b^2 + b'^2),$$

$$\tan \beta = -\frac{b'}{b}, \quad \tan \gamma = \frac{b'}{b}.$$

And therefore, if δ be determined by the equation

$$\tan \delta = \frac{b'}{b} = \frac{ka'}{ha} = \frac{k}{h} \tan \alpha, \quad \ldots \ldots \quad (4)$$

we see that, finally,

$$\left.\begin{array}{l} x' = m \cos (\theta + \delta), \\ y' = m \cos (\theta - \delta). \end{array}\right\} \quad \ldots \ldots \quad (5)$$

By any adequate action upon the reflected ray at any point between the iron mirror and the analyzer, let the component x' be retarded relatively to y', so as to undergo a relative change of phase equal to 2δ. As the components x' and y' have already equal amplitudes, and are equally inclined to O X, it is evident that by this change of phase of x' the elliptic vibration (5) is transformed into a rectilinear in the primitive direction O X. And thus the compensation of effect of the operation R is fully effected, without displacement of the second Nicol.

If we assign to $\frac{k}{h}$ the value $\tfrac{1}{2}$, which is probably near the truth, as its value in the case of steel, measured both by Jamin and by Senarmont, lies between ·5 and ·6, and if we give effect to the condition that α is a very small angle, we find from equation (4), approximately,

$$2\delta = 2 \tan^{-1} . \frac{k}{h} \tan \alpha = 2 \frac{k}{h} \alpha = \alpha.$$

However, it is sufficient for our present purpose to observe that the compensating change of phase 2δ is a small quantity determined by α, and of the same order as α, and also of the same sign.

16. *The Compensator.*—This is a slip of plate glass held in the hands, and strained either by flexure round its thickness, or by simple tension or compression from the two ends. In the present experiments the slips used were of the best plate,

$\frac{1}{8}$ inch thick, $\frac{9}{8}$ wide, and $7\frac{1}{2}$ long, chosen carefully so as to be quite inactive in the polariscope while unstrained. Suppose one of these slips placed between the mirror and the second Nicol, its surface perpendicular to the reflected ray, and its length parallel to O X′; and let the glass be stretched in the direction of its length. Stretched glass acts upon transmitted light as a positive uniaxal with its axis parallel to the line of extension. In this case, therefore, the extraordinary component x' is retarded relatively to the ordinary y'; and the method found in the last article gives us this simple rule :—

To compensate the effect of a small operation R or L, the incident vibration being initially directed along O X, at right angles to the plane of incidence, and the reflected vibration being initially cut off by the second Nicol. Leaving the second Nicol in its initial position, and placing the compensating slip between the mirror and the second Nicol, its plate faces perpendicular to the ray, and its length parallel to O X′; stretch the slip along its length in the case of R, and compress it along its length in the case of L.

The direction O X′ will be taken as the standard direction of strain : it is at 45° to the plane of reflection, right hand down.

17. *Third experiment.*—All the arrangements are as in the first experiment, the angle of incidence about 75°, and the extinction in the polariscope perfect. As the experiment is a purely optical one, the circuit is kept open. To ensure uniformity of optical conditions, the block C is kept in position as in the diagram of (7), and the light is viewed through the chink as formerly.

(1) The first Nicol is turned righthandedly through a very small angle, and the light is distinctly restored. The compensating slip is introduced between the mirror and the second Nicol in the manner which has just been fully described. When the slip is stretched along its length, say between closely gripping finger and thumb at each end, with a force which increases continuously from zero up to a certain small value, the light restored by displacement of the first Nicol fades away to pure extinction, reappearing and brightening as the tension increases. When the slip is submitted to a longitudinal compression which increases continuously from zero, the light increases continuously and very distinctly from beginning to end of the increase of compression.

(2) The first Nicol is now turned to the left, through the position of extinction, and the light distinctly restored; and the compensator, kept always in the standard position, is stretched and compressed as formerly. Things are precisely as in the first case, except that the effects of tension and com-

pression are reversed, and therefore interchanged. It is now compression that extinguishes the light; tension strengthens it from first to last.

When the angle of rotation of the first Nicol is too large, which it may be while still very small, the neutralization by tension or compression is incomplete, the light fading to a very sensible minimum and then increasing; but the extinction is still perfect when the initial intensities have reached much greater values than those obtained by magnetization in the first experiment.

I found the present experiment a very interesting one, from the simplicity of the means, the purity of effects, and the beautiful distinctness of the contrasts. However, I do not give the experiment here for its own sake. The only use of it is to characterize the effects of R and L in the polariscope; and this work it does perfectly.

18. *Fourth experiment.*—The angle of incidence about 75°, and all the arrangements and procedure as in the first experiment, with addition of the compensator. As the intensity in the polariscope is very faint at the best, all proper means are adopted for increasing it—the room well darkened, the battery in good order, the surface of the mirror fresh, the chink between wedge and core merely wide enough to give a good object, and the initial extinction sensibly perfect.

When the light is restored from pure extinction by the operation N, and the compensator is placed and strained as in the third experiment, the light is weakened by tension and strengthened by compression, and the weakening by tension proceeds to pure extinction. The effect of the operation S is, on the contrary, weakened to extinction by compression, and strengthened from first to last by tension.

This is the general result; but some precautions had to be taken in the actual experiment. Sometimes heat from the hand, possibly also from the breath, gave rise in the compensating slip to strains which had large effects in the polariscope, effects larger indeed than that to be compensated. In such a case the slip was laid aside and a fresh one employed. It was found necessary also to keep the plate faces of the slip accurately perpendicular to the reflected beam, as a very small displacement from this direction gave a noxious effect. Observing these and other precautions, and working with proper care, I found after some practice that the phenomena were perfectly under control. Sitting down in front of the polariscope, and getting an assistant to hold the submagnet and work the commutator, I bring the compensator suddenly into the standard position, and find the extinction still pure. The cir-

cuit is now closed, and the light reappears through the compensator. I now compress the slip along its length with a force increasing slowly from zero, and find that the light increases continuously and very distinctly as the compression increases. I therefore pronounce the mirror a north pole, which the assistant finds right. To verify by the polariscope, I stretch the slip with a force increasing slowly from zero, and find that the light fades to pure extinction and then brightens. The effects are very faint, but quite unmistakable. In the last-mentioned case, for instance, I put the light out of sight by careful increase of the tension up to a certain small value, and keep it out of sight as long as I please by sustaining the force, straining my eye all the time to catch the faintest glimpse, till the instant when the slip is relieved of strain without change of position ; and then the light reappears as at first. Working similarly in another case, I find these optical effects of tension and compression interchanged, compression extinguishing the light and tension strengthening it ; and the mirror is found accordingly to be a south pole.

19. *Fifth experiment.*—This is a repetition of the second experiment with addition of the compensator ; it is more easily managed than the fourth ; and the results are equally convincing. In the first half of the second experiment, as already described (10), the three sets of operations applied successively were

$$(R, N), \quad R, \quad (R, S);$$

and the intensities in the polariscope in the three cases respectively were bright, faint, dark.

When the effects in the first and second cases are tested by the compensator, exactly as in the third and fourth experiments, they are both compensated to pure extinction by tension, and both strengthened from first to last by compression. And similarly in the second half of the second experiment, the single effect of L and the joint effect of L and S are both strengthened by tension, and both weakened down to sensible extinction by compression.

20. Summary of the results obtained in the last three experiments.

The effects of the operations R and L in the polariscope are compensated respectively by tension and by compression of glass in the standard direction : the effect of N is compensated precisely as that of R, and the effect of S precisely as that of L ; the joint effect of R and N is compensated precisely as the separate effects of R and N, and the joint effect of L and S precisely as the separate effects of L and S : and in all these cases the compensation proceeds to sensible extinction.

The four operations (R, N), (L, S) were found in the second experiment to be related to one another, two and two, as conspiring or contrary; they are now seen to be related to one another more generally, and in the same combinations, as like or unlike. With reference to effects in the polariscope, the operations R and S are as clearly unlike as are the operations R and L, or the operations N and S; and, on the other hand, and with reference always to effects in the polariscope, R and N are as clearly like as are any two operations R, or any two operations N. It was assumed, in explanation of the facts brought out in the second experiment, that the optical effects of the four operations (R, N), (L, S) are the same in kind for all, and similarly directed for those of either pair, but oppositely directed for those of different pairs. All the new facts agree with this hypothesis and confirm it.

It has been observed already that the effects of the operations R and L are fully impressed upon the light before incidence, while the effects of N and S are impressed in the process of reflection; but, as far as we can judge from the present experiments (17, 18, 19), and as far as changes of phase of the principal components are concerned, this difference between the mechanical operations and the magnetic has little influence upon the final effect in the polariscope. We may therefore assume provisionally that, as far as changes of phase by metallic reflection are concerned, the rotation due to magnetic force is impressed effectively before incidence. We come now to the second method proposed in 14.

21. *Sixth experiment.*—Angle of incidence about 75°, initial arrangements as in the first experiment, plane of polarization of the incident light sometimes parallel and sometimes perpendicular to the plane of incidence, initial extinction as pure as possible.

(1) Leaving the first Nicol untouched, I turn the second Nicol righthandedly through a very small angle; and watching the faint light thus restored, I work the commutator as formerly. The operation N strengthens the light; and this effect is distinct and regular. The operation S has sometimes no effect, and sometimes weakens the light, always less distinctly than N strengthens it, and generally less and less distinctly as the rotation of the second Nicol is diminished.

(2) The second Nicol is turned to the left from its initial position through a very small angle. N and S now interchange effects; but otherwise the phenomena are as in the first case.

The effects obtained in repeated and careful trials were, with few exceptions, as I have now described them; but they were

neither so strong nor so pure as those obtained in the second experiment. The strengthening actions of N in (1) and of S in (2) are evidently what was to be expected; for in (1) the second Nicol leaves the plane of polarization a little to the left, and N turns that plane a little more to the left. But the whole subject deserves a more particular discussion.

22. To find the intensity of the light which reaches the observer's eye in the sixth experiment.

Suppose the incident vibration directed along O X (figure of art. 15), at right angles to the plane of incidence. When the second Nicol is turned (righthandedly) through a very small (positive) angle $Y O D = \epsilon$, the resolved part of the reflected vibration (of amplitude 1) in the direction O D has an amplitude $- \sin \epsilon$ or $-\epsilon$, and the intensity of the light transmitted to the eye is ϵ^2.

The effect of an additional operation S is to turn the primitive vibration out of the direction O X through a very small (positive) angle ρ, or to add to the primitive vibration in O X a very small vibration, of amplitude $\sin \rho$ or ρ, in a direction perpendicular to O X. There are therefore two vibrations presented now to the second Nicol—one in O X and sensibly of amplitude 1 as before, the other in O Y and of amplitude $k'\rho$ or ρ', where k' is a positive number less than 1, an unknown function of the angle of incidence. According to the hypothesis advanced in the end of art. 20, the difference of phases of these components has the same value ϕ as if the component ρ' in O Y were due to an operation R or L. The resolved parts of these components in the direction O D of transmission have amplitudes $- \sin \epsilon$ and $\rho' \cos \epsilon$, or $-\epsilon$ and ρ'; the intensity of the transmitted light is therefore equal to

$$\epsilon^2 + \rho'^2 - 2\epsilon\rho' \cos \phi.$$

23. Before discussing this formula, I proceed to apply similar considerations very briefly to the second experiment. Suppose the direction O X of the primitive vibration still perpendicular to the plane of incidence, and that positive angles are still those due to righthanded rotations. If two operations, L and S, be applied simultaneously, the vibration is turned through a small angle α before incidence, and through a small angle ρ in the process of reflection. The amplitudes of the small reflected vibrations thus generated in the direction O Y of transmission may be represented by α' and ρ', where α' is the $k\alpha'$ of equations (1) of art. 15, and ρ' is the same as in art. 22. According to the hypothesis stated in art. 20, these vibrations are reflected in the same phase, and the intensity of the transmitted light is therefore equal to $(\alpha' + \rho')^2$

To apply this result to the first half of the second experiment. By trial we give to α such a value that sensibly $\alpha' = \rho'$, and then apply successively the three sets of operations

$$(R, N), \quad (R), \quad (R, S).$$

The corresponding intensities in the polariscope are

$$(-\alpha' - \alpha)^2, \quad (-\alpha')^2, \quad (-\alpha' + \alpha')^2,$$

which are as the numbers 4, 1, 0. The actual results, as already specified, were bright, faint, black (10).

24. Returning to the sixth experiment. In discussing the expression

$$\epsilon^2 + \rho'^2 - 2\epsilon\rho' \cos \phi,$$

found in art. 22, I shall suppose the rotation of the second Nicol always righthanded, or the angle ϵ always positive. The amplitude ρ' is positive for S, negative for N. The angle ϕ varies continuously with the angle of incidence, from zero at normal incidence, through $\frac{\pi}{2}$ at principal incidence (75° or 76°), up to π at grazing incidence. It will be observed that the $\frac{\pi}{2}$ at principal incidence in the present case is the $\frac{3}{2}\pi$ of equation (2) of art. 15, diminished by the π of reversal due to reflection.

(1) When the value of the angle of incidence is considerably less than 75°, $\cos \phi$ has some positive value c, and the additions made to the primitive intensity ϵ^2 by the operations N and S are

$$\rho'^2 + 2\epsilon\rho'c \text{ and } \rho'^2 - 2\epsilon\rho'c.$$

In this case, therefore, the effect of N in the polariscope is always an increase, and always more pronounced than the effect of S.

Let ϵ' be the value of ϵ which is determined by the equation

$$\rho' - 2\epsilon c = 0.$$

When $\epsilon = \epsilon'$, the effect of S in the polariscope is reduced to zero; when $\epsilon < \epsilon'$, the effect of S is a small increase; when $\epsilon > \epsilon'$, the effect of S is a decrease.

(2) When the value of the angle of incidence is considerably greater than 75°, $\cos\phi$ has some negative value $-e$, and the additions made to the primitive intensity ϵ^2 by N and S are

$$\rho'^2 - 2\epsilon\rho'e \text{ and } \rho'^2 + 2\epsilon\rho'e.$$

Here, therefore, contrary to what holds in the first case, the effect of S is always an increase, and always more pronounced than the effect of N. Here also, as ϵ increases from zero, the

addition made to ϵ^2 by the weaker magnetic operation passes from positive, through zero, to negative.

(3) In the case of principal incidence, $\cos \phi = 0$, and the additions made by N and S to the primitive intensity ϵ^2 in the polariscope are equal and always positive.

25. *Seventh experiment*, a repetition of the sixth, to test the preceding inferences.

(1) Angle of incidence about 70°. All the effects recovered as predicted, and as already obtained roughly in the sixth experiment. Recovered also perfectly at various incidences from 60° to 75°.

(2) Angle of incidence very large, about 85°. No sensible effect obtained in any case by application of the operations N and S, with the arrangements of the sixth experiment, or with those of the second. The reason very probably is that, as the angle of incidence approximates to 90°, the ratio of the amplitudes ρ' and ϵ becomes excessively small, by diminution of the rotation ρ towards zero.

Angle of incidence about 80°. The effects very faint, but clearly contrary to what was predicted : N strengthens the light as in the first case ; S either weakens it or has no effect.

(3) Equal positive effects of N and S in the polariscope were never observed at 75° or any other incidence. The hypothesis advanced in 20 is therefore inexact : the rotation due to magnetic force is not impressed effectively before incidence. Neither is it impressed effectively after reflection (10 ... 19). The difference of phases of the two reflected vibrations, ρ' in O Y, and 1 in O X, has therefore some value $\lambda \phi$ intermediate between ϕ and 0 ; and the intensity in the sixth experiment is equal to

$$\epsilon^2 + \rho'^2 - 2\epsilon\rho' \cos \lambda\phi.$$

Judging from the earlier experiments, second to fifth, I think we are bound to assume that λ is very nearly equal to 1: but certainty upon the subject can be reached only through exact measurements. I come now to the third method mentioned in art. 14.

Case of Perpendicular Incidence.

26. *Submagnet.*—The old wedge C of art. 7 is now inadequate. The piece which I substitute for it is a block of soft iron, 2 inches square and 3 inches long, rounded at one end into a frustum of a very obtuse cone, of which the small base is hardly $\frac{1}{2}$ inch in diameter. A small boring is drilled through the block, and along the axis of the cone, narrowing regularly from $\frac{1}{4}$ inch at the flat end of the block to $\frac{1}{12}$ inch

at the conical end. The surface of the boring is well dimmed
with a coating of lampblack. To ensure perfect stability of
position when the piece rests upon its conical end, the original
rectangular volume of the block was restored, the part added
being a hard stone of plaster of Paris, which was easily
moulded to the block of iron in the usual way. This is the
first submagnet that gave me good and constant effects in the
case of normal incidence ; and it appears to be much the best
that I have yet tried.   Without a submagnet of some kind, I
have never obtained a suspicion of an effect.

27. *Placing of the pieces.*—The old magnet (3) is placed
on a solid table near the edge, with its polar surfaces horizon-
tal ; and the submagnet just described is laid upon one of the
polar surfaces, its conical end downwards, the axis of core
and boring coincident, and the block and core separated by a
wide ring of writing-paper, or very thin card.   Above the
block, as in Norremberg's polariscope, stands a mirror of un-
silvered glass, which receives a horizontal beam from the first
Nicol, and reflects it downwards through the boring, perpen-
dicularly to the surface
of the magnetic mirror.
In the diagram, H F G
is the polar surface, L
the source of light, which
is the same paraffin-flame
as formerly, E the ob-
server's eye, A and B the
first and second Nicols,
C the transparent mir-
ror.   The course of the
light from L to E is
L A C F C B E.   All the

pieces are placed very stably, and the room is well darkened.
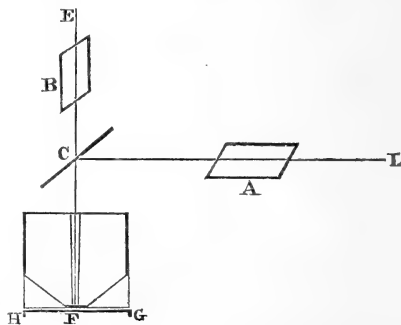
28. *Eighth experiment.*—All the pieces are placed as in
the diagram, and so that the observer sees at F, through B, a
bright and steady image of part of the flame L : the first
Nicol is so laid that the plane of polarization of the light in-
cident at C coincides with the plane of incidence ; and the
second Nicol is turned into the position of pure extinction.

(1) The second Nicol is turned righthandedly through a
small angle, giving a distinct but faint restoration.   The
operation N strengthens the light thus restored; and the
operation S weakens and sometimes extinguishes it.

(2) The second Nicol is turned lefthandedly through a
small angle beyond pure extinction.   The results are as in
the first case, with reversal of actions of N and S.   It is now

S that strengthens the light, and N that weakens or extinguishes it.

The phenomena now mentioned are very faint, a good deal fainter than those obtained in the second experiment; but they are certain, distinct, and perfectly regular. I need hardly say that this experiment is decisive, and that the effects are certainly due to rotations virtual and actual of the plane of polarization of the light which is presented to the analyzer, the virtual rotations being produced by displacements of the second Nicol, and the actual by the operations N and S. N conspires with a righthanded rotation of the second Nicol; and therefore N turns the plane of polarization to the left: S conspires with a lefthanded rotation of the second Nicol; and therefore S turns the plane of polarization to the right.

29. *Ninth experiment.*—No change in the arrangements, the initial extinction perfect.

(1) The first Nicol is turned righthandedly (from C as point of view) through a small angle, giving a faint but distinct restoration. S strengthens the light thus restored, and N weakens and sometimes extinguishes it.

(2) The first Nicol is turned lefthandedly, through pure extinction, to faint restoration. N strengthens the restored light, and S weakens or extinguishes it.

The phenomena are precisely as in the eighth experiment, and equally distinct and regular, but with reversed relations of S and N to movements of the Nicol: and this was to be certainly expected, because the first Nicol simply carries the plane of polarization with it, while the second Nicol simply leaves that plane behind it. As an illustration of this statement, and of the consistency of the results obtained in this article and the preceding, I offer the following optical experiment, though it will be to many of my readers unnecessary.

30. *Tenth experiment.*—The arrangements unchanged, the initial extinction perfect, the circuit kept open.

(1) First Nicol to the right, giving a faint restoration. The restored light is weakened to extinction by rotation of the second Nicol to the right; strengthened clearly, *ab initio,* by rotation of the second Nicol to the left.

(2) First Nicol to the left, giving a faint restoration. The light is weakened to extinction by rotation of the second Nicol to the left; strengthened clearly, *ab initio,* by rotation of the second Nicol to the right.

These effects are certain and regular; but sensibly perfect extinctions are obtained only in careful work, and with very small displacements of the first Nicol.

31. *Eleventh experiment.*—Starting with the same arrange-

ments as in the last three experiments, and working under the most favourable conditions attainable, I have often left the two Nicols in position at pure extinction, and tried the effects of the simple operations N and S. I have certainly got distinct effects many times in such circumstances, and assured myself that they were due to magnetizations of the iron mirror by getting them to appear and disappear at the instants of make and break of the circuit ; but the effects were so excessively faint that I could not once characterize them as due to rotation of the plane of polarization. I have no doubt whatever that, with a stronger magnet and a finer mirror, and a more intense light, this experiment would be as satisfactory as any of the preceding.

32. *Twelfth experiment : influence of the Submagnet.* — The old wedge C of art. 7 has a slit sawn into it at right angles to the edge, as if to divide the block into two equal wedges. The slit is about $\frac{1}{12}$ inch wide, and terminates at the dotted line drawn across the block C in the diagram of 7. Returning to the diagram of art. 27, the bored block is removed, and the slit block put in its place, its largest plane face on the polar surface, and the slit perpendicular to the plane L C F. Block and core are separated successively by six sheets of increasing thickness, tissue-paper, thin writing-paper, drawing-paper, pasteboard, thick card-board, and a quarter-inch plank, each of the sheets being perforated properly at F, so as to expose the polar surface through the slit. All the other arrangements and the procedure are as in the eighth and ninth experiments. The old effects are obtained under these new conditions, but more faintly at the best. They are certainly strongest with the sheets of pasteboard and card-board, $\frac{1}{30}$ inch to $\frac{1}{12}$ inch thick. With the quarter-inch plank they are barely if at all perceptible. With the first and second sheets, the tissue-paper and thin writing-paper, I could catch no trace of the effects.

Summary of Experimental Results.

33. When plane-polarized light is reflected perpendicularly from the polar surface of an iron electromagnet, the plane of polarization is turned through a small angle in a direction contrary to the nominal direction of the magnetizing current.

When the light is reflected obliquely, the effect in the polariscope is mixed, partly due to magnetic force, and partly due to metallic reflection ; but in this case, as evidently as in the case of normal incidence, the action of the magnetic force is purely or chiefly photogyric, and the plane of polarization

is turned always in a direction contrary to that of the mag-
netizing current.

The precise character of the mixed optical effect in the
case of oblique incidence can be determined only by exact
measurements. This much, however, appears to be clearly
proved by the preceding experiments, that the rotation due
to magnetization of the mirror is impressed upon the light
neither effectively before incidence, nor effectively after re-
flection.

No effect was obtained in any case without the presence of
a submagnet. I think it certain that the only use of this
piece is to concentrate or intensify the magnetic force upon
the iron mirror by inductive action.

The powers applied were barely adequate to produce all the
effects. Some of the phenomena were quite imperceptible
when the battery began to work, and afterwards, when it had
worked at intervals for three or four hours. Much better
effects may certainly be expected with higher electromagnetic
powers and finer optical appliances.

Glasgow, 26th March, 1877.

XLIV. *Notes on the Theory of Sound.* By R. H. M.
Bosanquet, *Fellow of St. John's College, Oxford.*

[Continued from p. 278.]

2. *On the Energy per second of a Pendulum-vibration in Air.*

THE flow of energy per second along a column of air
transmitting a pendulum-vibration is

$$2\rho v^3 \left(\frac{\pi A}{\lambda}\right)^2, \text{ or } 2 \times 1 \cdot 4 \Pi v \left(\frac{\pi A}{\lambda}\right)^2,$$

where ρ is the density of the air,
Π the atmospheric pressure,
v the velocity of sound,
A amplitude of vibration,
λ wave-length.

This was proved in a paper in the Philosophical Magazine,
March 1873. The kinetic and potential energy were esti-
mated separately; each is equal to half the above quantity.

This result may be obtained more conveniently by supposing
a disk to oscillate in an infinite cylinder. The changes of pres-
sure on the two sides of the disk are always equal and opposite;
and the work done in any small movement is the product of
the displacement by the difference of the pressures. The total
work done by the disk is the energy supplied to keep up the

two streams of sound which are propagated from it in both directions.

Let the axis of the cylinder be the axis of x, the origin the position of rest of the disk, and y the displacement at any point.

Then, on the right, where the transmission is forward,

$$y_1 = A \sin \frac{2\pi}{\lambda}(vt-x),$$

$$\frac{dy_1}{dx} = -\frac{2\pi A}{\lambda}\cos\frac{2\pi}{\lambda}(vt-x).$$

And on the left, where the transmission is backward

$$y_2 = A \sin \frac{2\pi}{\lambda}(vt+x),$$

$$\frac{dy_2}{dx} = \frac{2\pi A}{\lambda}\cos\frac{2\pi}{\lambda}(vt+x).$$

And the difference of pressures on the two sides of the disk, where $x=0$, is

$$1\cdot4\Pi\left(\frac{dy_2}{dx}-\frac{dy_1}{dx}\right)_{x=0},$$

or, since $\dfrac{v}{\lambda}=\dfrac{1}{\tau}$,

$$2\times1\cdot4\,\Pi\times2\,\frac{\pi A}{\lambda}\cos2\pi\frac{t}{\tau}$$

The work done by the disk in passing through the space dy against this difference of pressures is this difference $\times dy$, and

$$\left(\frac{dy}{dt}\right)_{x=0}=\frac{2\pi A}{\tau}\cos2\pi\frac{t}{\tau}\,;$$

$\therefore$ work done in a quarter vibration is

$$1\cdot4\Pi\frac{8\pi^2 A^2}{\lambda\tau}\int_0^{\frac{\tau}{4}}\cos^2 2\,\frac{\pi t}{\tau}\,dt = 1\cdot4\Pi\,\frac{\pi^2 A^2}{\lambda}\,;$$

$\therefore$ work in a whole vibration

$$= 4\times1\cdot4\Pi\frac{\pi^2 A^2}{\lambda}\,;$$

and in a second, since vibration-number $=\dfrac{v}{\lambda}$,

$$= 4\times1\cdot4\Pi v\left(\frac{\pi A}{\lambda}\right)^2.$$

And this work produces two streams of pendulum-waves in

air of amplitude A, so that the work supplied to each stream per second is

$$2 \times 1{\cdot}4 \Pi v \left(\frac{\pi A}{\lambda} \right)^2,$$

as before.

Cor.—Since the maximum velocity in the vibration is $2\dfrac{\pi A v}{\lambda} = V$ say, we may write the energy per second,

$$2 \rho v^3 \left(\frac{\pi A}{\lambda} \right)^2 = \tfrac{1}{2} \rho v V^2.$$

Here ρv is the mass of air which the sound traverses in a second.

Hence the energy transmitted in a given time is the same that the whole mass of air traversed would have if moving with the maximum velocity of the vibration.

This is in analogy with the general theorem, that the energy of a body executing pendulum-vibrations is the same as if the mass were arranged on the circumference of a fly-wheel whose radius is the amplitude, and period that of the vibration—a representation of the motion which is frequently convenient.

The maximum velocity is the velocity of the circumference of such a wheel; or $v = 2\pi A n$. It is consequently independent of the velocity of transmission.

Cor.—Since the velocity of sound in a gas is subject[*] to the equation

$$v^2 \rho = 1{\cdot}4 \Pi,$$

where v, ρ may vary, but Π is constant, we have, putting $\rho = \dfrac{1{\cdot}4 \, \Pi}{v^2}$ in the last expression, energy per second

$$= \frac{1{\cdot}4}{2} \frac{\Pi}{v} V^2.$$

Hence, in the transmission of a vibration of given amplitude and vibration-number through any gas, the energy per second is inversely as the velocity of sound in the gas, or is proportional to the square root of the density.

This is the case of replacing air by hydrogen in a receiver with a bell hung in it. So long as there is only air present, the energy per second is simply proportional to the pressure; but if hydrogen be introduced, the velocity of propagation is increased, and the energy per second diminished in inverse ratio. Ultimately when the air is replaced by hydrogen, the energy per second is $\frac{1}{3{\cdot}8}$ of its value for air.

* If, like hydrogen, it have the same ratio of specific heats as air.

Cor.—Since the maximum pressure in a transmitted vibration is

$$1 \cdot 4 \Pi \frac{2\pi A}{\lambda} = P \text{ say,}$$

and

$$\frac{2\pi A}{\lambda} = \frac{V}{v},$$

$$\therefore \ P = 1 \cdot 4 \Pi \frac{V}{v}.$$

The energy per second may then be written

$$\tfrac{1}{2} PV.$$

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3. On the Reflexion of Sound at Gaseous layers of different Density[*].

We confine ourselves to the case of a cylinder, which we suppose to be crossed by layers of gas of different density.

Consider first one common surface, and let the velocity of propagation on the side from which the air comes be v, on the other side v'.

There will be generally a reflected and a transmitted disturbance; let

the amplitude of the incident disturbance be A,
    „   „   reflected   „ a,
    „   „   transmitted   „ b.

The common surface moves with the sum of the amplitudes A, a of the one side, and with the amplitude b of the other;

$$\therefore \ A + a = b.$$

The energy per second of A $=$ that of a, b together;

$$\therefore \ 2 \times 1 \cdot 4 \Pi \pi^2 n^2 \left\{ \frac{A^2}{v} = \frac{a^2}{v} + \frac{b^2}{v'} \right\};$$

or if $\dfrac{v}{v'} = x$,

$$A^2 = a^2 + xb^2.$$

Putting $b^2 = (A + a)^2$, we find

$$A^2(1 - x) = a^2(1 + x) + 2Aax;$$

and, rejecting the solution $A = -a$, $b = 0$, we have

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[*] General formulæ for a single surface were given by Green. The object of this communication is chiefly to give an example of the method. The case of oblique incidence loses most of its interest in practical application to sound, as the necessary conditions for formal refraction are not generally satisfied; the investigation has been restricted to the simpler case.

$$a = A\frac{1-x}{1+x}, \quad b = \frac{2A}{1+x};$$

or, replacing x by $\frac{v}{v'}$,

$$a = A\frac{v'-v}{v'+v}, \quad b = \frac{2Av'}{v'+v}.$$

The ratio of energy transmitted : original is

$$\frac{\dfrac{b^2}{v'}}{\dfrac{A^2}{v}} = \frac{v}{v'}\frac{4v'^2}{(v+v')^2} = \frac{4vv'}{(v'+v)^2}.$$

The ratio of energy reflected : original is

$$\frac{a^2}{A^2} = \left(\frac{v'-v}{v'+v}\right)^2.$$

Since v, v' are symmetrically involved, the division takes place in the same way whether we pass from the denser to the rarer, or from the rarer to the denser medium.

Let

$$\frac{4vv'}{(v'+v)^2} = \alpha, \quad \left(\frac{v'-v}{v'+v}\right)^2 = \beta.$$

Suppose the sound passes across a layer of gas or heated air (two surfaces); then the following represents the distributions of the sound-energy after reflexions and transmissions at the two surfaces of the layer :—

Back.	Within the layer.	Through.
Refl. β	Trans. α	α^2,
Trans. $\alpha^2\beta$	Refl. back $\alpha\beta$,	
	Refl. forward $\alpha\beta^2$...	$\alpha^2\beta^2$,
Trans. $\alpha^2\beta^3$	Refl. back $\alpha\beta^3$,	
	Refl. forward $\alpha\beta^4$...	$\alpha^2\beta^4$,

and so on.

The total transmission is $\dfrac{\alpha^2}{1-\beta^2}$.

Total reflexion back is $\left(\dfrac{\alpha^2}{1-\beta^2}+1\right)\beta$.

Putting for α, β their values,

$$\frac{\alpha^2}{1-\beta^2} = \frac{(4vv')^2}{(v+v')^4 - (v-v')^4} = \frac{2vv'}{v^2+v'^2} \text{ trans.}$$

$$\left(\frac{\alpha^2}{1-\beta^2}+1\right)\beta = \left(\frac{2vv'}{v^2+v'^2}+1\right)\left(\frac{v-v'}{v+v'}\right)^2 = \frac{(v-v')^2}{v^2+v'^2} \text{ refl.}$$

The sum of these results $=1$, as it should be. In these calculations it is supposed that the wave is of such length compared with the thickness of the layer as to prevent interference-effects.

Example.—Sound is transmitted across a layer of hydrogen; to find the energy per second transmitted and reflected.

Density of hydrogen : density of air $: : v^2 : v'^2 : : 1 : 14\cdot44$ nearly;

whence $v : v' : : 1 : 3\cdot8$ nearly.

Calculating the above expressions, we find that, if energy per second of incident sound be taken as 1, it is divided as follows :—

$$\text{Transmitted} = \frac{2 \times 3\cdot8}{1+14\cdot44} = \frac{7\cdot60}{15\cdot44}.$$

$$\text{Reflected} \quad = \frac{(3\cdot8-1)^2}{1+14\cdot44} = \frac{7\cdot84}{15\cdot44}.$$

The transmitted and reflected energy are very nearly equal; and each is very nearly half that of the incident sound.

We can find the value of $v : v'$ for which the division should be exactly equal between transmission and reflexion.

Putting $(v-v')^2 = 2vv'$, we find

$$\frac{v'}{v} = 2 \pm \sqrt{3} = 3\cdot732 \text{ or } \cdot268,$$

the one corresponding to passage from air through gas, the other to the reverse. This value is within the limits marked by the square root of the density and Regnault's determination of the velocity in hydrogen.

Regnault's determination.	Square root of density.
3·682	3·801

(These numbers are given correctly in Regnault, *Mém. de l'Institut*, 1868, p. 135. They are accidentally transposed at p. 553 of the memoir, and also in the summary in Tyndall, 2nd edit. p. 331.)

As an illustration of the case where the wave-length is not such as to prevent interference, suppose that we are dealing with a layer of the thickness of a quarter wave-length in the gas. Then each successive term of both transmissions and reflexions is the opposite phase from that which precedes it, so that the total energy transmitted is less than the above.

The calculations, of which the above is an example, furnish the explanation of Tyndall's observations about the acoustic opacity of aerial layers of different density.

The observation that the conditions of transmissibility are

not the same for high and low sounds is probably to be accounted for by the fact that the relations which produce interference are not the same in the two cases. As a matter of fact, high sounds are most affected in general by transmission through obstacles or layers of any kind. The reason is, no doubt, that under ordinary circumstances the dimensions considered are comparable with short wave-lengths rather than with long ones.

The method given above can be extended to any number of layers by a process analogous to one used in calculating the effect on light of a pile of glass plates.

It is only necessary to remark that the layer behaves like a single surface whose dividing ratios are

$$\alpha' = \frac{\alpha^2}{1-\beta^2}, \quad \beta' = \left(\frac{\alpha^2}{1-\beta^2}+1\right)\beta \ ;$$

or, remembering that $\alpha + \beta = 1$,

$$\alpha' = \frac{\alpha}{2-\alpha}, \quad \beta' = 1-\alpha'.$$

Thus for two layers,

$$\alpha_2 = \frac{\alpha'}{2-\alpha''}, \quad \beta = 1-\alpha_2.$$

$$= \frac{\alpha}{4-3\alpha}.$$

and for 2^n layers,

$$\alpha_{2^n} = \frac{\alpha}{2^n-(2^n-1)\alpha}, \quad \beta_n = 1-\alpha_n.$$

XLV. *Reply to the Note of* Professor M. Avenarius.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the February Number of the Philosophical Magazine, page 156, Professor M. Avenarius does me the honour to notice a communication of mine "On a Mechanical Illustration of Thermo-electric Phenomena" (December Supplement, 1876), wherein I attributed incorrectness to his formula for the electromotive force of a thermo-electric couple,

$$E = (t_1 - t_2)\{b + c(t_1 + t_2)\}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

an imputation which Professor Avenarius rejects.

Now it is true that this expression, taken by itself and independently of the process by which it was obtained, is not erroneous but quite correct—being, in fact, as he justly

observes, of the very same form as the one subsequently given by Professor Tait (1870–71) to express the value of this electromotive force. A few words of explanation are therefore necessary from me.

The work of Professor Avenarius in 1863, as detailed in his memoir*, " Die Thermoelektricität, ihrem Ursprunge nach, als identisch mit der Contactelektricität betrachtet," consisted, first of all, in arriving at this expression by means of two hypotheses, very reasonable and probable at first sight, but really inconsistent with each other, and one of them false ; and secondly, in bringing the expression so obtained to the test of experiment, and verifying it in a complete and satisfactory manner between certain limits of temperature. This second part of his work is unassailable ; and as it was performed several years before the similar more extensive experiments of Professor Tait, it constitutes, so far as I know, the original discovery of the actual numerical laws of the electromotive force of a thermo-electric pair.

Regarded, then, as an empirical formula expressing with experimental accuracy the electromotive force of a thermoelectric circuit, it is correct ; but considered as a formula deduced from and embodying a certain theory in contact-electricity (which theory one would naturally have supposed to be verified by the verification of the formula), it is erroneous, and has led Professor Avenarius himself to erroneous† results. In a subsequent communication to Poggendorff's *Annalen*‡ in 1873, to which he now refers me, he admits this to some extent ; and in this paper he makes full use of the laws discovered thermodynamically by Sir William Thomson in 1851, and shows that his own expression is in agreement with them if the " specific heat of electricity in a metal " is assumed to be proportional to its absolute temperature. Professor Tait had pointed this out in a very similar manner a few years previously § ; but the original publication of the substance of the 1873 paper took place in the Reports of the University of Kiew for 1870, a copy of which Professor Avenarius has been good enough to send me, as I was unable to find them in the library of the British Museum. Hence it appears that the priority in this also rests with him.

* Pogg. *Ann.* vol. cxix.
† See, for instance, Pogg. *Ann.* vol. cxxii., where the Volta contact-force between two metals at any temperature is supposed to be deduced with the help of thermo-electric measurements, the two distinct phenomena of voltaic and thermal electromotive force being mixed up and confused together.
‡ Vol. cxlix. "Ein Beitrag zur Theorie der Thermoströme."
§ Proc. Roy. Soc. Edinb. 1870–71.

The errors in the original theory are rather subtle, and are worth pointing out, not for the purpose of casting any slur on the valuable work of Professor Avenarius, but as illustrating the slight though important difference between the present theory which includes Thomson's effects, and the two hypotheses which ignored them and which were also independent of the laws of thermodynamics.

First hypothesis. That the electromotive force of a thermal joint is expansible in terms of the (Centigrade) temperature, thus

$$e = a + bt + ct^2. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Second hypothesis. That in a complete circuit there is no electromotive force anywhere except at the junctions of different metals, or that the electromotive force of a thermo-electric pair, with two joints at different temperatures, is obtained simply by subtracting the electromotive force of the joint at the temperature t_2 from that of the joint at the temperature t_1, or

$$\mathrm{E} = e_1 - e_2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Conclusion following from these, the law of Avenarius—

$$\mathrm{E} = b(t_1 - t_2) + c(t_1^2 - t_2^2). \quad . \quad . \quad . \quad (3)$$

Now the first hypothesis taken by itself is a tolerably safe assumption, and, as it happens, is actually true as it stands. But when the second hypothesis is also made, the general theory of heat-engines requires the constant c to be zero, which would render most of the subsequent investigation meaningless, and would make (3) entirely discordant with experiment. This, in fact, is the very discrepancy which led Sir William Thomson about 1851 or 1852 to see the falsehood of hypothesis No. 2, and hence to the discovery of an electromotive force between different portions of one and the same metal at different temperatures, which, under the form of the "electric convection of heat," he subsequently caused to be verified by a most laborious series of experiments [*].

The falsehood of the second hypothesis is fully admitted by Professor Avenarius in 1873 [†].

The conclusion when considered alone is correct, as I have said above; but the constants b and c have not the same meaning as they had in (1). If we consider them to have the same value in (1) and (3), we shall be led into error as regards matters of fact, just as Professor Avenarius was.

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[*] Phil. Trans., Bakerian Lecture, 1856. Oddly enough this memoir is quoted by Professor Avenarius in the original paper where the Thomson-effects are ignored.

[†] Pogg. *Ann.* vol. cxlix. footnote, page 374.

Thus from (1) we observe that e vanishes for two particular temperatures, one of which may be absolute zero, -274; and then the other will be $274 - \dfrac{b}{c}$, which is Thomson's "neutral point," and may be denoted by t_0. It attains a maximum value when $t = -\dfrac{b}{2c}$, which may be called t_m.

From (3) it is plain that E vanishes when $t_1 = t_2$, and also when

$$\frac{t_1 + t_2}{2} = -\frac{b}{2c} = t_m.$$

(This temperature t_m is what Professor Avenarius prefers to call the neutral point; but that is immaterial.)   We are thus led to conclude that the vanishing-point of E coincides with the temperature giving a maximum value to e, or that the Peltier effect at a junction of two metals would be greatest for a temperature halfway between two temperatures for which the electromotive force of a complete circuit vanishes; whereas the fact is that there is no Peltier effect at all at this halfway temperature, and E really vanishes when $\frac{1}{2}(t_1 + t_2)$ equals t_0, and not when it equals t_m.   Moreover the metals do change places in the thermo-electric series at the mean temperature where E vanishes, a fact which Professor Avenarius (consistently with his hypothesis) would deny *.

If we now amend the second hypothesis so as to include Thomson's effect and start from hypothesis (1), we shall arrive at the correct value for E in terms of the constants of (1); but it will not be identical with (3).   Let us write z instead of the number 274 (or more generally let z be the absolute temperature corresponding to the zero of the scale employed) and then proceed thus.   Thermo-electromotive force at a junction of two metals whose Centigrade temperature is t,

$$e = a + bt + ct^2. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Difference of electromotive forces at two such junctions whose temperatures are t_1 and t_2 respectively,

$$e_1 - e_2 = b(t_1 - t_2) + c(t_1^2 - t_2^2).$$

Difference of electromotive forces *in* the two metals when there is a difference of temperature, $t_1 - t_2$, between their ends (Thomson's effect),

$$\epsilon_a - \epsilon_b = -zc(t_1 - t_2) - \tfrac{1}{2}c(t_1^2 - t_2^2). \quad . \quad . \quad (4)$$

Electromotive force in entire circuit of the two metals,

$$E = e_1 - e_2 + \epsilon_a - \epsilon_b = (b - zc)(t_1 - t_2) + \tfrac{1}{2}c(t_1^2 - t_2^2). \quad . \quad (5)$$

* Cf. p. 413, vol. cxix. Pogg. *Ann.*

This being the correct expression in terms of the constants of
(1), one is justified in saying that (3) is erroneous—although,
as (5) and (3) *happen* to be of precisely the same form, one
is just as good as the other if the constants are arbitrary and
have no special meaning assigned to them.

In conclusion it may be worth while just to write down the
physical meaning of the three constants a, b, and c,

$$a = z\beta t_0 = zb - z^2 c, \quad b = \beta(t_0 - z), \quad c = -\beta, \quad . \quad . \quad (6)$$

where t_0 stands for the temperature of the neutral point,

$$t_0 = z - \frac{b}{c}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and β stands for J times the difference of the specific heats of
electricity in the two metals expressed as a function of the
absolute temperature and divided by the temperature.   Or, in
Avenarius's (1873) notation,

$$\beta = \beta_1 - \beta_2.$$

In Tait's,

$$\beta = k_a - k_b.$$

In Thomson's,

$$\beta = J \frac{\sigma_a - \sigma_b}{T}.$$

By the help of (6) we may remove the constant a from (1)
and write it

$$e = b(z + t) + c(t^2 - z^2). \quad . \quad . \quad . \quad (8)$$

The temperature for which e is a maximum is

$$t_m = -\frac{b}{2c} = \tfrac{1}{2}(t_0 - z).$$

The following Table gives the correct values of a, b, and c
for a few junctions, according to the (1863) experiments ot
Professor Avenarius, taking z as equal to 274.

Metals.	$a.$	$b.$	$c.$	$t_0.$
Silver and iron	902·6	— ·734	−·0147	224
Copper and iron	264·4	+ ·006	−·0035	275
Silver and zinc	−81·9	+ ·879	+·0043	70
Platinum and lead ...	23·3	+2·606	+·0092	−9

The values of t_0 are explicitly inserted in order to show that
the relation $a = -zct_0$ is tolerably well satisfied.

I am, Gentlemen,

Your obedient servant,

University College, London.   OLIVER J. LODGE.

XLVI. *Notice of Crystallographical Forms of Glaucodote.* By
W. J. Lewis, *M.A., Fellow of Oriel College, Assistant in the
Mineral Department, British Museum* *.

[Plate I.]

THE mineral from Häkansbo in Sweden is found in large
crystals of metallic lustre and dull tin-white colour,
imbedded in towanite and pyrites. The crystals are, for the
most part, twins, most of them being twinned about the normal
to the face (0 1 1), so far not described, some about the nor-
mal to the prism-face (1 1 0). The specific gravity and the
prism-angle agree fairly well with those of Breithaupt's
acontite.

	Glaucodote.	Acontite (Br.).
Sp. gr.	5·985–6·18.	6·008–6·059.
Prism-angle . . .	69° 32′	69° 31′
Brachydome-angle .	100° 2′	102° 0′

The one discrepancy consists in the value of the angle of
the brachydome {1 0 1}, which from direct observation and a
calculation by the method of least squares involving all the
best angles measured, I make out to be 100° 2′. The angle
of the prism varies considerably in different crystals—68° 57′,
69° 6⅔′, 69° 13′, 69° 32′, 69° 40′ having been obtained on
fairly good specimens. The planes of the brachydome are
much striated, and do not allow of such precise determination.
Differences in the brachydome-angle, such as those found in
the prism-angle, have been observed, though much more limited
in extent. This variation of the crystallographic elements of
the mineral is probably to be accounted for by a variation of
the quantity of cobalt present. The angle of the prism (=67°
24′) and the cleavage *c*, given in Miller's 'Mineralogy,' seem
to belong to some other mineral. They were not determined
by Professor Miller himself; and I have been unable to find
out whence they are taken. On the very large crystals are
found the forms {0 1 0}, {1 1 0}, {1 0 1}, {1 0 2}, {0 1 1} ; on
smaller crystals have been found the additional forms, {1 1 1},
{1 2 2}, {2 0 1}. Fig. 1 (Plate I.) is a stereographic projection
of these forms. Such simple crystals as I have observed were
extended considerably in the direction of the edge of the bra-
chydome, but were all broken at one end. On one of these,
whose prism-angle measured 69° 19½′, a second prism {1 6 0}
was observed. Its faces were small and not well developed.
Assuming the measured angle 69° 19½′ for the fundamental

* Communicated by the Crystallological Society, having been read
December 15, 1876.

prism, calculation gives the angle $(1\,1\,0,\,1\,6\,0)=27°\ 56\frac{1}{4}'$, which agrees very well with that obtained by measurement, $27°\ 54\frac{1}{2}'$.

The elements and angles given below have been taken from measurements obtained on two good twin-crystals, each about the size of a cherry.

System prismatic :

$D=(0\,1\,0,\,0\,1\,1)=30°\ 12\frac{1}{3}'$; $E=(0\,0\,1,\,1\,0\,1)=50°\ 1'$;

$F=(1\,0\,0,\,1\,1\,0)=55°\ 14'$.

$$a:b:c=1\cdot4406:1:1\cdot71784.$$

Forms observed:

$b\{0\,1\,0\}$, $m\{1\,1\,0\}$, $y\{2\,0\,1\}$, $l\{1\,0\,1\}$, $s\{1\,0\,2\}$, $n\{0\,1\,1\}$,

$o\{1\,1\,1\}$, $w\{1\,2\,2\}$, $p\{1\,6\,0\}$.

	Calculated.		Found.	
$m\,m_,$ =	69	32	69	32
$l\,l_,$	100	2	100	1
$l\,s$	19	$12\frac{3}{4}$	19	0
$l\,y$	17	14	17	13
$y\,y_,$	134	30	134	$31\frac{1}{2}$
$l\,y_,$	62	44	62	$43\frac{2}{3}$
$m\,y$	58	$16\frac{1}{2}$	58	15
$m\,l$	64	$5\frac{1}{2}$	64	$4\frac{1}{2}$
$n\,w$	16	42	16	$43\frac{1}{2}$
$w\,w_,$	33	24	33	27
$o\,w$	14	$15\frac{2}{3}$	14	7
$m\,n$	44	46		
$n\,l$	71	$8\frac{1}{3}$		
$m\,\mu$	89	32	89	36
$m\,\lambda$	26	$22\frac{1}{3}$	26	32

The observed angles agree fairly well with those obtained by calculation, with the exception of $l\,s$ and $m\,\lambda$. The observation of the latter was one of no great weight; but the former was repeatedly measured with great care. As moreover an increase of $5'$ in the angle E involves an increase in $l\,s$ of $\frac{3}{4}'$ only, I have been obliged to regard the discrepancy as due to a distortion of the face s in the crystal on which the angle was measured. This face s, though large, is generally one of the worst on the crystal. The plane n is but poorly developed, and therefore does not serve for a direct determination of the element D. The plane b is deeply striated parallel to its intersection with m; the planes l and s are striated parallel to their intersections with one another, s being much the rougher.

2 A 2

The planes y are pitted and rough.   Figs. 2 and 3 are representations of simple crystals.

The twins about (0 1 1) are a fresh illustration of the tendency to twin about the face of a prism whose angle is near 60°.   Fig. 4 is a representation of this twin, in which both members are shown approximately *in æquilibrio* with the twin-axis vertical.   Fig. 5 represents somewhat closely the appearance of a moderately large twin of this kind.   The principal crystal, denoted by Roman letters, is projected in the same way as in fig. 1; and to it are attached two smaller crystals twinned about (1̄ 1 0), the one represented by Greek letters, the other by barred Greek letters.   The intersection of planes which correspond is straight and definite, of planes which do not correspond (as s and μ) is irregular and indefinite.   The elements obtained by measurements on this crystal differ slightly from those given above, as is shown by the following Table:—

	Calculated.	Found.
D	$= 30°\ 14\frac{1}{2}'$	
E	$50\ \ 2\frac{2}{3}$	
F	$55\ 10$	
$m\,m_{,}$	$69\ 40$	$69°\ 38\frac{1}{2}'$
$s\,s_{,}$	$61\ 40$	$61\ 34\frac{1}{2}$
$l\,s$	$19\ 12\frac{2}{3}$	$19\ 14\frac{3}{4}$
$m\,l$	$64\ \ 2$	$64\ \ 1\frac{1}{2}$
$m\,s$	$72\ 58\frac{1}{2}$	$72\ 53$
$m\,\mu_{,}$	$48\ 50\frac{1}{3}$	$48\ 59$

One specimen in the British Museum consists of a triple twin, resembling those of chrysoberyl, two of the members being twinned on adjacent faces of the form {0 1 1} of the third. Fig. 6 is an orthogonal projection on the plane (1 0 0) of this twin, in which an attempt has been made to show the appearance of the specimen and the relative magnitude of the members.   Fig. 6 a shows the simple crystal in the same projection.

Twins about the face of the prism {1 1 0} have been already observed.   These twins generally show the tendency to develop but slightly in the direction of the twin axis; and I was fortunate enough to get a specimen showing this to a remarkable degree.   It is about the size of a penny-piece, and about the thickness of the thick penny of George III.   One member is about half the width of the other, the remainder being apparently untwinned without an increase of its thickness. Close inspection, however, shows the existence of twin laminæ in this part.

XLVII. *Supplementary Note to* Professor Des Cloizeaux's *Memoir on Humite* (Phil. Mag. [V.] vol. ii.). *By* Professor A. Des Cloizeaux†.

SINCE the communication of this memoir to the Crystallological Society, I have resumed the examination of the optical character of a certain number of crystals of the three types of Vesuvian humite which I received from Professor Scacchi. The results obtained are similar to those which I have already published ; but the crystals of type II. are especially remarkable for the great number of bands twinned round the normal to the base, to which the bands are parallel. In consequence of this twinning, a face a^1, for example, is formed very frequently by a portion of a^1 occupying its proper position, and by a portion of $o^{\frac{1}{2}}$ belonging to a twin band. The same coincidence occurs with the corresponding faces $a^{\frac{2}{3}}$ and $o^{\frac{1}{6}}$, $a^{\frac{1}{2}}$ and h^1, b^1, and $d^{\frac{3}{4}}$, &c. Now, no reentering angle being observed on these compound faces, their incidences on the base should be equal. It is precisely what one remarks in the calculated angles contained in the following Table, and which have been obtained by substituting for two of the elemental angles, which I have borrowed from Scacchi, the following:

$*p\,h^1 = 109° 1'$ (vom Rath) instead of $108° 58'$ (Scacchi).

$*pd^{\frac{1}{2}} = 125° 50'$,,   ,, $125° 52'$,,

$b : a : c :: 419058 : 907949 : 696666.$

$p\,e^2$	141	50	$p\,a_2$	114	58	$p\,b^1$	135	19
$p\,e^1$	122	28	$p\,\beta$	95	19	$p\,d^{\frac{3}{4}}$	135	19
$p\,a^1$	135	57	$p\,\dot{\eta}$	125	2	$p\,b^{\frac{3}{4}}$	125	50
$p\,o^{\frac{1}{2}}$	135	57	$p\,o_3$	125	2	$*p\,d^{\frac{1}{2}}$	125	50
$p\,a^{\frac{2}{3}}$	119	52	$p\,a_3$	103	9	$p\,b^{\frac{1}{2}}$	113	26
$p\,o^{\frac{1}{6}}$	119	52	$g^1\,a_3$	135	41	$p\,d^{\frac{1}{4}}$	113	26
$p\,a^{\frac{1}{2}}$	109	1	$*g^1\,h^3$	135	41	$p\,m$	98	13
$*p\,h^1$	109	1						

XLVIII. *Note on the Law of Twinning and Hemihedrism of Leucophane.* *By* Emile Bertrand, *Paris*†.

THE crystallography of leucophane has been treated of by Professor Des Cloizeaux in his *Manuel de Minéralogie*, by Mr. Greg (Phil. Mag. [IV.], vol. ix. p. 510), and by Professor

† Communicated by the Crystallological Society, having been read April 12, 1877.

Von Lang (*Mineral. Mittheil. Tschermak*, 1871). I have also published a note on this mineral in the *Annales des Mines*, vol. iii. 1873 ; but up to the present time two interesting facts concerning it have not been noticed.   It is known that leucophane has a very good cleavage parallel to the base, $p = 0\ 0\ 1$, and that the acute bisectrix of the optic axes is perpendicular to this cleavage.   With a sufficiently large plate it is easy to obtain with the polarizing microscope two directions apparently at 90° to one another, in which the hyperbolæ and the lemniscates are perceived.

In certain cases one finds two crystals separated by a single plane of junction which have the planes of their optic axes orientated at about 90° to one another.   In other cases one observes a series of very narrow bands corresponding to as many crystals of which the axes are situated in planes making angles of nearly 90° with one another.   By an examination of a great number of cleavages of leucophane, I have succeeded in establishing that these twins are very frequent, especially in plates of some magnitude.   I shall describe a very good twin crystal of this kind, which, moreover, presents another peculiarity.   It consists of two crystals placed at about 90° to one another, the two bases lying in one and the same plane. Each of these crystals shows two well-developed faces, $b^1 = 1\ 1\ 2$, truncating two edges parallel to the base ; the two other edges are not truncated in the large crystal ; but in the smaller one faces $b^{\frac{1}{2}} = 1\ 1\ 1$ are found truncating these edges.
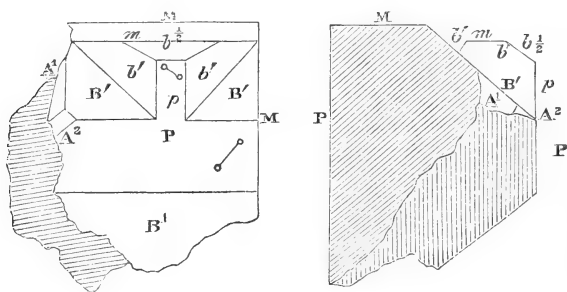
Is, then, leucophane hemihedral, and indeed doubly so, as is the case with edingtonite ?   To prove this, the observation of a large number of crystals will be necessary ; and unfortunately crystals of leucophane are very rare.   I have examined a crystal in the collection of Mr. Adam, which presents the same hemihedrism : two edges parallel to the base are modified by $b^1 = 1\ 1\ 2$, and one of the two others by $b = 1\ 1\ 1$.

The fourth edge is not present, owing to the crystal being broken at this part.

The British Museum also contains a crystal which I have not examined; but the figure of the crystal published in Tschermak's *Mineral. Mittheil.* (1871) shows that the crystal is modified differently on the four edges of the base. This crystal therefore seems to present the same hemihedrism as that noticed above. Without pronouncing definitely on the hemihedrism, I think that there is sufficient probability to justify me in calling attention to the fact, and to encourage an examination as to whether other crystals present the same hemihedrism.

Instead of considering leucophane to be hemihedral, one might suppose that it belonged to the oblique system rather than to the prismatic. On this supposition the base would become the plane of symmetry $g^1 = 0\ 1\ 0$, the bisectrix of the optic axis would be the horizontal diagonal of the base, the faces $b^1 = 1\ 1\ 2$ and $b^{\frac{1}{2}} = 1\ 1\ 1$ would become $m = 1\ 1\ 0$ and $e^4 = 1\ 0\ 4$ respectively. But this supposition would only be admissible if the hemihedrism to which I have called attention were a hemihedrism with parallel faces; for in the case of a hemihedrism with inclined faces one would not find the four vertical faces necessary for forming the prism. But the twinned specimen of which I have spoken above shows very clearly a hemihedrism with inclined faces. Moreover, if leucophane crystallized in the oblique system one would probably observe a crossed dispersion in the polarizing microscope, whereas the symmetry of the dispersion of the colours is most perfect. This does not constitute a proof, but renders it extremely probable that the system is prismatic.

I have likewise proved that the trace of the plane of the optic axes on the face $p = 0\ 0\ 1$, parallel to the cleavage, coincides with the diagonal of this face; for if a twinned plate be examined with the ordinary microscope between two Nicols, the line of separation of the two crystals is observed to be situated very approximately at 45° to the trace of the plane of the optic axes of each of the two crystals, the plane of the optic axes of one crystal being at 90° to that of the other.

This coincidence, which is necessary in the prismatic system, but not in the oblique, gives a fresh argument in favour of the system being prismatic.

We may therefore conclude that the greater part of the crystals of leucophane are twinned and most probably hemihedral. It will be interesting to obtain evidence whether other crystals besides those mentioned above present the same hemihedrism.

XLIX. *Extension of a Theorem in Continuants, with an important application.* By Thomas Muir, *M.A., F.R.S.E.*[*]

IN the Philosophical Magazine for February there appeared, with a demonstration, a solitary theorem in continuants, which was thought worth placing on record on account of its intrinsic neatness, and not because of any known application to the subject on which continuants bear. Now, however, I am enabled not only to make a generalization of it, but to apply it in demonstrating an extension of an important theorem of Professor Bauer's regarding the product of two continued fractions.

Beginning with the continuant

$$\begin{vmatrix} \delta + \dfrac{b_1}{r} & b_1 c_1 & 0 & 0 & 0 \\ -1 & \delta + \dfrac{b_2}{r} - c_1 r & b_2 c_2 & 0 & 0 \\ 0 & -1 & \delta + \dfrac{b_3}{r} - c_2 r & b_3 c_3 & 0 \\ 0 & 0 & -1 & \delta + \dfrac{b_4}{r} - c_3 r & b_4 c_4 \\ 0 & 0 & 0 & -1 & \delta + \dfrac{b_5}{r} - c_4 r, \end{vmatrix}$$

we transform it first into

$$\begin{vmatrix} \delta + \dfrac{b_1}{r} & b_1 & 0 & 0 & 0 \\ -c_1 & \delta + \dfrac{b_2}{r} - c_1 r & b_2 & 0 & 0 \\ 0 & -c_2 & \delta + \dfrac{b_3}{r} - c_2 r & b_3 & 0 \\ 0 & 0 & -c_3 & \delta + \dfrac{b_4}{r} - c_3 r & b_4 \\ 0 & 0 & 0 & -c_4 & \delta + \dfrac{b_5}{r} - c_4 r. \end{vmatrix}$$

Now, as before, increasing the elements of the first, second, ... rows by r times the corresponding elements of the second, third, ... rows, we obtain for the continuant an expression in non-continuant form; then, in this third form, diminishing the elements of the second column by r times the corresponding elements of the first column, diminishing the elements of the third column by r times the corresponding elements of the *new* second column, and so on, there results

$$\begin{vmatrix} \delta + \dfrac{b_1}{r} - c_1 r & b_2 & 0 & 0 & 0 \\[2mm] -c_1 & \delta + \dfrac{b_2}{r} - c_2 r & b_3 & 0 & 0 \\[2mm] 0 & -c_2 & \delta + \dfrac{b_3}{r} - c_3 r & b_4 & 0 \\[2mm] 0 & 0 & -c_3 & \delta + \dfrac{b_4}{r} - c_4 r & b_5 \\[2mm] 0 & 0 & 0 & -c_4 & \delta + \dfrac{b_5}{r} \end{vmatrix},$$

which, lastly, is equivalent to

$$\begin{vmatrix} \delta + \dfrac{b_1}{r} - c_1 r & b_2 c_1 & 0 & 0 & 0 \\[2mm] -1 & \delta + \dfrac{b_2}{r} - c_2 r & b_3 c_2 & 0 & 0 \\[2mm] 0 & -1 & \delta + \dfrac{b_3}{r} - c_3 r & b_4 c_3 & 0 \\[2mm] 0 & 0 & -1 & \delta + \dfrac{b_4}{r} - c_4 r & b_5 c_4 \\[2mm] 0 & 0 & 0 & -1 & \delta + \dfrac{b_5}{r} \end{vmatrix}.$$

Taking, therefore, continuants of the nth order, we have as a general theorem,

$$\left.\begin{aligned} &\mathrm{K}\left(\delta + \frac{b_1}{r} \;\overset{b_1 c_1}{} \delta + \frac{b_2}{r} - c_1 r \;\overset{b_2 c_2}{} \delta + \frac{b_3}{r} - c_2 r \ldots \overset{b_{n-1} c_{n-1}}{} \delta + \frac{b_n}{r} - c_{n-1} r \right) \\ =\;&\mathrm{K}\left(\delta + \frac{b_1}{r} - c_1 r \;\overset{b_2 c_1}{} \delta + \frac{b_2}{r} - c_2 r \;\overset{b_3 c_2}{} \delta + \frac{b_3}{r} - c_3 r \ldots \overset{b_n c_{n-1}}{} \delta + \frac{b_n}{r} \right). \end{aligned}\right\} (\mathrm{A})$$

On putting $c_1 = c_2 = c_3 = \ldots = 1$ and writing β for $\delta - r$, this becomes

$$\left.\begin{aligned} &\mathrm{K}\left(\beta + \frac{b_1}{r} + r \;\overset{b_1}{} \beta + \frac{b_2}{r} \;\overset{b_2}{} \beta + \frac{b_3}{r} \ldots \overset{b_{n-1}}{} \beta + \frac{b_n}{r} \right) \\ =\;&\mathrm{K}\left(\beta + \frac{b_1}{r} \;\overset{b_2}{} \beta + \frac{b_2}{r} \;\overset{b_3}{} \beta + \frac{b_3}{r} \ldots \overset{b_n}{} \beta + \frac{b_n}{r} + r \right); \end{aligned}\right\} (\alpha)$$

which is the theorem given in the February Number, notwithstanding the great apparent difference in the two ways of expressing it. In fact such theorems, as happens in the majority of cases in determinants, are better expressed in words. Stating (α) in this way, we affirm that " If in any continuant the ratio of the excess of any element of the minor diagonal over the preceding element to the similar excess in the case of the

elements of the main diagonal which occupy the same rows be constant, except in the case where the first element of all is concerned, and even then the condition hold when this element is diminished by the constant referred to, then the continuant is unaltered in value by diminishing the first element by the constant, increasing the last by the same, making the elements of the minor diagonal advance a place, and filling up the vacant last place by the element which would have followed had the continuant been of a higher order." A similar statement of (A) would be more complicated, but may without much difficulty be framed on the same model.

Having thus established a relation (of equality) between two continuants, we should expect to find a relation between the two continued fractions corresponding to two such continuants. To this end let us consider the continued fraction

$$\frac{b_1(b_2-\alpha)}{\beta} + \frac{b_2(b_3-\alpha)}{\beta} + \cdots + \frac{b_{n-1}(b_n-\alpha)}{\beta},$$

which

$$= \frac{b_1(b_1-\alpha)\mathrm{K}(\beta \ ^{b_3b_4-b_3\alpha}\beta \ ^{b_4b_5-b_4\alpha} \cdots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)}{\mathrm{K}(\beta \ ^{b_2b_3-b_2\alpha} \beta \ ^{b_3b_4-b_3\alpha} \cdots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)} = \mathrm{F} \text{ say.}$$

Using the elementary theorem that

$$\mathrm{K}(x_1 \ ^{z_1} \ x_2 \ ^{z_2} \ x_3 \ ^{z_3} \ x_4 \ldots) = x_1 \mathrm{K}(x_2 \ ^{z_2} \ x_3 \ ^{z_3} \ x_4 \ldots) + z_1 \mathrm{K}(x_3 \ ^{z_3} \ x_4 \ldots),$$

we have

$$(b_2-\beta)\mathrm{K}(\beta \ ^{b_3b_4-b_3\alpha} \beta \ldots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)$$
$$+ \mathrm{K}(\beta \ ^{b_2b_3-b_2\alpha} \beta \ldots \ ^{b_{n-1}-b_nb_{n-1}} \beta)$$

$$= (b_2-\beta)\mathrm{K}(\beta \ ^{b_3b_4-b_3\alpha} \beta \ldots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)$$
$$+ \beta\mathrm{K}(\beta \ ^{b_3b_4-b_3\alpha} \beta \ldots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)$$
$$+ (b_2b_3-b_2\alpha)\mathrm{K}(\beta \ ^{b_4b_5-b_4\alpha} \beta \ldots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)$$

$$= b_2\mathrm{K}(\beta \ ^{b_3b_4-b_3\alpha} \beta \ldots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)$$
$$+ (b_2b_3-b_2\alpha)\mathrm{K}(\beta \ ^{b_4b_5-b_4\alpha} \beta \ldots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)$$

$$= b_2\mathrm{K}(\beta+b_3-\alpha \ ^{b_3b_4-b_3\alpha} \beta \ldots \ ^{b_{n-1}b_n-b_{n-1}\alpha} \beta)$$

$$= b_2\mathrm{K}(\beta+b_3-b_4 \ ^{b^2_4-b_4\alpha} \beta+b_4-b_5 \ ^{b^2_5-b_5\alpha} \ldots \ ^{b^2_n-b_n\alpha} \beta+b_n-\alpha)$$

by means of (A).

Again,

$$(b_2-\alpha)\mathrm{K}(\beta \; {}^{b_3 b_4 - b_3 \alpha}\; \beta \ldots {}^{b_{n-1} b_n - b_{n-1}\alpha}\; \beta)$$
$$+ \mathrm{K}(\beta \; {}^{b_2 b_3 - b_2 \alpha}\; \beta \ldots {}^{b_{n-1} b_n - b_{n-1}\alpha}\; \beta)$$

$$= (b_2-\alpha)\mathrm{K}(\beta \; {}^{b_3 b_4 - b_3 \alpha}\; \beta \ldots {}^{b_{n-1} b_n - b_{n-1}\alpha}\; \beta)$$
$$+ \beta\mathrm{K}(\beta \; {}^{b_3 b_4 - b_3 \alpha}\; \beta \ldots {}^{b_{n-1} b_n - b_{n-1}\alpha}\; \beta)$$
$$+ (b_2 b_3 - b_2 \alpha)\mathrm{K}(\beta \; {}^{b_4 b_5 - b_4 \alpha}\; \beta \ldots {}^{b_{n-1} b_n - b_{n-1}\alpha}\; \beta)$$

$$= (\beta + b_2 - \alpha)\mathrm{K}(\beta \; {}^{b_3 b_4 - b_3 \alpha}\; \beta \ldots {}^{b_{n-1} b_n - b_{n-1}\alpha}\; \beta)$$
$$+ (b_2 b_3 - b_2 \alpha)\mathrm{K}(\beta \; {}^{b_4 b_5 - b_4 \alpha}\; \beta \ldots {}^{b_{n-1} b_n - b_{n-1}\alpha}\; \beta)$$

$$= \mathrm{K}(\beta + b_2 - \alpha \; {}^{b_2 b_3 - b_2 \alpha}\; \beta \; {}^{b_3 b_4 - b_3 \alpha}\; \beta \ldots {}^{b_{n-1} b_n - b_{n-1}\alpha}\; \beta)$$

$$= \mathrm{K}(\beta + b_2 - b_3 \; {}^{b_3{}^2 - b_3 \alpha}\; \beta + b_3 - b_4 {}^{b_4{}^2 - b_4 \alpha} \ldots {}^{b_n{}^2 - b_n \alpha}\; \beta + b_n - \alpha) \text{ by (A).}$$

Now the ratio of the two expressions here transformed becomes on dividing by $\mathrm{K}(\beta^{b_2 b_3 - b_2 \alpha}, \beta \ldots \beta)$,

$$\frac{(b_2-\beta)\dfrac{\mathrm{F}}{b_1(b_2-\alpha)} + 1}{\dfrac{\mathrm{F}}{b_1} + 1};$$

and the ratio of the two results is

$$\cfrac{b_2}{\beta + b_2 - b_3 +} \cfrac{b_3(b_3-\alpha)}{\beta + b_3 - b_4 +} \cdots \cfrac{}{\cdots + } \cfrac{b_n(b_n-\alpha)}{\beta + b_n - \alpha}.$$

Hence we have an identity of the kind surmised (*i. e.* connecting two continued fractions), viz.

$$\cfrac{b_2(b_2-\alpha)}{\beta + b_2 - b_3 +} \cfrac{b_3(b_3-\alpha)}{\beta + b_3 - b_4 +} \cdots \cfrac{}{\cdots + } \cfrac{b_n(b_n-\alpha)}{\beta + b_n - \alpha} = \frac{(b_2-\beta)\mathrm{F} + b_1(b_2-\alpha)}{\mathrm{F} + b_1}. \text{ (B)}$$

F being the continued fraction given above.

Increasing both sides of this by $\dfrac{\beta+\alpha}{2} - b_2$, we have

$$\frac{\beta+\alpha}{2} - b_2 + \cfrac{b_2(b_2-\alpha)}{\beta + b_2 - b_3 +} \cfrac{b_3(b_3-\alpha)}{\beta + b_3 - b_4 +} \cdots \cfrac{}{\cdots + } \cfrac{b_n(b_n-\alpha)}{\beta + b_n - \alpha} = \frac{\alpha-\beta}{2} \cdot \frac{\mathrm{F} - b_1}{\mathrm{F} + b_1}.$$

Now the fraction F remains unchanged when the sign of α and the signs of all the b's are changed; hence, making this substitution, there results

$$\frac{\beta-\alpha}{2}+b_2+\cfrac{b_2(b_2-\alpha)}{\beta-b_2+b_3+}\cfrac{b_3(b_3-\alpha)}{\beta-b_3+b_4+}\ \cdots\ +\cfrac{b_n(b_n-\alpha)}{\beta-b_n+\alpha}=-\frac{\alpha-\beta}{2}\cdot\frac{F+b_1}{F-b_1};$$

and therefore by multiplication we have the remarkable result

$$\left.\begin{array}{l}\dfrac{\beta+\alpha}{2}-b_2+\cfrac{b_2(b_2-\alpha)}{\beta+b_2-b_3+}\cfrac{b_3(b_3-\alpha)}{\beta+b_3-b_4+}\ \cdots\ +\cfrac{b_n(b_n-\alpha)}{\beta+b_n-\alpha}\\[2ex]\times\\[1ex]\dfrac{\beta-\alpha}{2}+b_2+\cfrac{b_2(b_2-\alpha)}{\beta-b_2+b_3+}\cfrac{c_3(b_2-\alpha)}{\beta-b_3+b_4+}\ \cdots\ +\cfrac{b_n(b_n-\alpha)}{\beta-b_n+\alpha}\\[2ex]=\dfrac{\beta^2-\alpha^2}{4},\end{array}\right\}\ (\mathrm{C})$$

where it is easily seen in what cases the fractions may be continued *ad infinitum*.

Putting $\alpha=0$ in this, we have Professor Bauer's [*] first case; and his second case is obtained on putting $\alpha=b-c$, and $b_2, b_3, b_4 \ldots = b,\ b+h,\ b+2h,\ldots$ respectively. Further, if after these last substitutions we write $2s+h$ for β, and $h-b$ for c, there results Euler's less general theorem [†]; and if from this we specialize still further by taking $s=a-1$, $h=2$, and $b=1$, we find the old well-known identity of Wallis [‡], used in the establishment of Brounker's expression for $\frac{\pi}{4}$, viz.

$$\left.\begin{array}{l}a+1+\cfrac{1}{2(a+1)+}\cfrac{3^2}{2(a+\mathrm{I})+}\cfrac{5^2}{2(a+1)+}\ \cdots\\[2ex]\times\\[1ex]a-1+\cfrac{1}{2(a-1)+}\cfrac{3^2}{2(a-1)+}\cfrac{5^2}{2(a-1)+}\ \cdots\end{array}\right\}=a^2.$$

Returning to (B) and taking b_2 from both sides, we have

[*] *Von einem Kettenbruche Euler's u. s. w.* München, 1872.
[†] *Comment. Acad. Petropol.* vol. xi. 1739, p. 57, § 46.
[‡] *Arithmetica Infinitorum,* Prop. CXCI.

$$-b_2 + \frac{b_2(b_2-\alpha)}{\beta+b_2-b_3} + \frac{b_3(b_3-\alpha)}{\beta+b_3-b_4} + \cdots + \frac{b_n(b_n-\alpha)}{\beta+b_n-\alpha} = -\frac{\beta F + \alpha b_1}{F + b_1}; \quad (\beta)$$

therefore also, as before,

$$b_2 + \frac{b_2(b_2-\alpha)}{\beta-b_2+b_3} + \frac{b_3(b_3-\alpha)}{\beta-b_3+b_4} + \cdots + \frac{b_n(b_n-\alpha)}{\beta-b_n+\alpha} = -\frac{\beta F + \alpha b_1}{F - b_1}. \quad (\gamma)$$

Hence we have the curious theorem

$$\left.\begin{aligned}
&1 + \frac{b_2-\alpha}{\beta-b_2+b_3} + \frac{b_3(b_3-\alpha)}{\beta-b_3+b_4} + \cdots + \frac{b_n(b_n-\alpha)}{\beta-b_n+\alpha} \\
\div\; &-1 + \frac{b_2-\alpha}{\beta+b_2-b_3} + \frac{b_3(b_3-\alpha)}{\beta+b_3-b_4} + \cdots + \frac{b(b_n-\alpha)}{\beta+b_n-\alpha} \\
=\; &1 + \frac{b_2-\alpha}{\beta} + \frac{b_3(b_3-\alpha)}{\beta} + \cdots + \frac{b_{n-1}(b_n-\alpha)}{\beta} \\
\div\; &-1 + \frac{b_2-\alpha}{\beta} + \frac{b_3(b_3-\alpha)}{\beta} + \cdots + \frac{b_{n-1}(b_n-\alpha)}{\beta}.
\end{aligned}\right\} \quad (D)$$

Again, denoting the continued fractions in (β) and (γ) by f and f' respectively, we obtain from (C),

$$\left(\frac{\beta+\alpha}{2}+f\right)\left(\frac{\beta-\alpha}{2}+f'\right) = \frac{\beta^2-\alpha^2}{4};$$

whence

$$\frac{\beta-\alpha}{2}f + \frac{\beta+\alpha}{2}f' + ff' = 0,$$

and

$$\therefore \quad \frac{\alpha-\beta}{f'} - \frac{\alpha+\beta}{f} = 2. \quad \cdots \cdots \quad (E)$$

It would be hard perhaps to find a better illustration than is afforded by the foregoing of the great assistance which is derived from continuants in the investigation of the subject of continued fractions. Wallis's theorem was left by him unproved; and the demonstration of it, as Professor Bauer points

out, taxed the vast analytic power of Euler himself.   In two different memoirs* it is a subject of inquiry with him ; and in one place † he says that he had spent much labour upon it, but that the harder it seemed the more advantage did he hope to draw from the solution.   And yet Wallis's theorem is, as we have seen, one of the simplest cases of the general result here established with comparative ease.

Glasgow, February 6, 1877.

L. *Contributions to the Theory of Luminous Flames.*
By Dr. KARL HEUMANN ‡.
[Plate II.]
[Concluded from p. 107.]

IN a former part of these papers I have declared my belief in the view which regards the separation of solid carbon as the cause of the luminosity of the flame produced by burning hydrocarbonaceous bodies.   I have now to prove experimentally the justice of this belief, and to demonstrate the existence of free carbon in such flames.

The increase in the " *light-effect* " of a gas-flame occasioned by heating the burner-tube has been already traced to the increase in the *intensity of light* of the flame-mantle, and to the simultaneous enlargement of the flame itself.   The increase in intensity of the light may be itself traced either to the higher temperature to which the carbon particles are raised, or to the production of a greater number of such particles in a given volume of the flame-mantle.   In either case more light will be emitted by any special portion of the flame. Whether both causes are at work when the burner-tube is heated must remain meanwhile undecided.   The increase in size of the flame-mantle, noticed when the burner-tube is heated, has been referred to an earlier separation of carbon in the flame, this separation becoming possible by reason of the high temperature to which the gas has been raised.

If this explanation be the true one, it is manifest that any agent, other than heat, capable of producing a separation of carbon in the comparatively cold lower portions of the flame, should also be capable of producing an increase in the size of the flame-mantle.   Chlorine and bromine are known to be

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* " De Fractionibus Continuis Observationes," *Comment. Acad. Petropol.* vol. xi. 1739, pp. 32–81 ; " De Fractionibus Continuis Wallisii," *Mémoires de l'Acad. de St. Pétersbourg,* vol. v. 1815, pp. 24–44.

† *Comment. Acad. Petropol.* 1739, p. 41.

‡ Translated, and somewhat condensed, from Liebig's *Annalen,* vol. clxxxiv. pp. 206–254, by M. M. Pattison Muir.

capable of producing the former action ; and experiment shows that they can also produce the latter*.

Coal-gas issuing from a rather narrow horizontal tube was ignited ; the flame was separated by a space of 1 or 2 centims. from the orifice of the tube. A tube from which chlorine issued was introduced between the burner and the flame : the luminosity of the latter was at once increased ; and the flame at the same time extended itself backwards to the point at which the chlorine entered. The volume of the flame-mantle was thus increased at the expense of the blue zone of the flame.

If bromine be employed in place of chlorine, a sooty flame is produced ; this is to be traced to the formation of a gaseous compound of bromine and carbon, and consequent partial cutting off of the supply of oxygen.

Before deducing a wide generalization, it seemed to me necessary to inquire whether in *every* case introduction of chlorine brought about the result theoretically foretold.

The flame of a hydrocarbon may become feebly luminous (1) when the flame-mantle contains a small number of solid carbon particles. This condition is fulfilled in the flame of all substances relatively poor in carbon—for instance, in the flame of ordinary coal-gas. Such flames are rendered more luminous by addition of chlorine or bromine either before or after the gas is ignited. If chlorine be added before ignition, a partial combustion takes place in the innermost portions of the flame ; part of the hydrogen unites with the chlorine, while carbon is set free in solid form and renders luminous the hydrochloric-acid flame within the main flame. Combustion of carbon, and of the hydrogen which is uncombined with chlorine proceeds at the outer part of the flame ; inasmuch, however, as the outer portions are comparatively poor in hydrogen but rich in carbon, it follows that an increase in luminosity must take place at these points also.

If chlorine be conducted into the centre of a somewhat large coal-gas flame, two flames are easily distinguishable. If chlorine be mixed with the outer atmosphere in which combustion is taking place, it partially replaces not only the inert nitrogen, but also the oxygen, combining at the same time with hydrogen, but not at all, or only to a very limited extent, with carbon. The latter is therefore partially deposited as soot.

Gases containing little carbon may therefore be caused to burn with a feebly luminous flame by mixing with them substances which at a high temperature partially or completely

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* Berzelius showed that the flame of alcohol is rendered luminous by the introduction of chlorine. See Gmelin-Kraut's ' Handbook,' i. pt. 2, p. 13 ; 6th ed.

combine with the hydrogen present, and so produce a hydrocarbon rich in carbon, or set free pure carbon itself.

The substitution-products of marsh-gas, CH_3Cl and $CHCl_3$, are gases fulfilling these conditions.   While marsh-gas burns with a feebly luminous flame, the flame of methyl chloride (CH_3Cl) is smoky and strongly luminous.   Chloroform ($CHCl_3$) also burns at the surface of a wick with a luminous flame.   Hydrogen containing a little chloroform vapour burns with a brilliant although non-smoky flame ; if a large quantity of chloroform be present, the flame becomes somewhat opaque and deposits much soot.   In each of these cases hydrochloric acid is produced.

The flame of a hydrocarbon may become feebly luminous, or even non-luminous when (2) the temperature is not sufficiently high to cause separation of solid carbon.   Two cases here present themselves.   A low temperature may be occasioned by withdrawal of heat by extraneous causes, as when a luminous flame is brought into contact with cold substances, or by the action of admixed gases which absorb heat, and so reduce the original temperature of the flame.   These flames may be so hot as to cause to glow a piece of platinum wire held within them, and yet not hot enough to bring about a deposition of carbon from the hydrocarbonaceous material of the gas.   On the supposition already put forward, chlorine ought in either case to render the non-luminous flame luminous.

A small luminous flame was rendered non-luminous by causing it to play upon a porcelain basin.   (I have already shown that withdrawal of heat is here the cause of non-luminosity.)   So soon as chlorine or bromine vapour was brought into the blue flame, it became luminous and deposited soot upon the basin.   This experiment proves that the flame became non-luminous because the temperature was not attained at which carbon is deposited, and that so soon as carbon was separated, even by other means than increase of temperature, luminosity returned.

The blue flame of mixed coal-gas and carbon dioxide, coal-gas and air, or coal-gas and carbon monoxide, when mixed with a little chlorine became very luminous ; this was especially noticeable with the flame of mixed coal-gas and air, because this flame is possessed of a higher temperature than the others ; and I have already shown that the temperature at which deposition of carbon takes place in flames admixed with indifferent gases is higher than that at which the same phenomenon occurs in the case of flames not so admixed.

The luminosity of a hydrocarbon flame may be diminished (3) by the temperature not being sufficiently high to maintain

the separated carbon in such a condition as that it shall emit light. Such flames (the flame of turpentine for instance) become luminous when their temperature is increased ; this may be done by admitting air or oxygen. The admission of chlorine to such flames may be shown experimentally to have no effect in increasing luminosity.

Frankland has put forward the suggestion that the soot deposited from luminous flames does not consist of carbon, but of a mixture of heavy hydrocarbons whose vapours have been condensed upon the cold body introduced within the flame. Stein * has pointed out that in this case increase of temperature should cause the soot to again assume the gaseous form ; experiment proves that this is not so. The absorptive power of carbon for gases seems to me to explain the fact that the soot deposited from luminous flames does not consist of pure carbon. Stein's analyses show 99·1 per cent. of carbon and 0·9 per cent. of hydrogen. I have shown that admission of chlorine to flames containing decomposable hydrocarbons causes an increase in the luminosity of these flames, and that this increase is attended with deposition of soot. Can it be supposed that this soot consists of condensed heavy hydrocarbons ? Frankland has himself told us that to obtain pure carbon from the soot deposited from luminous flames it is necessary to heat the deposit in chlorine. If chlorine be then capable of decomposing hydrocarbons at a red heat with production of pure carbon, it can scarcely be the means of bringing about the formation of heavy hydrocarbons in the flame itself. In the case of flames rendered luminous by admission of chlorine, free carbon is evidently separated ; and as the phenomena attending the luminosity of flames of high temperature present no points of difference from the same phenomena in the case of flames containing chlorine, the conclusion is that in the high-temperature flames free carbon is also separated.

It has been already shown that when a porcelain rod is held in a gas-flame, the lower surface (that is, the surface opposed to the stream of burning gas) is alone at first covered with soot, and that a thin film of soot is formed on the upper surface only after the expiry of a considerable time.

This experiment affords direct proof of the presence of solid carbon particles in the luminous flame. If the action of the cold object were to condense the vapours of hydrocarbons, such condensation would of course take place equally around the cold object ; but the facts of the experiment show that the deposition is a purely mechanical operation exactly comparable with the deposition of dust upon the walls of a room.

* *Journ. Pract. Chem.* (N. S.) vol. viii. p. 402.

Further, the fact experimentally proved, that the surface of a body heated to redness may become covered with soot, is opposed to Frankland's hypothesis : if the deposition consisted of condensed hydrocarbons, it could only take place upon surfaces relatively colder than the flame itself.   If the space immediately over the flame of burning turpentine be examined, it is seen to contain flaky particles of sooty matter.   A hydrogen-flame brought into this layer becomes surrounded by a continuous luminous mantle ; the flame of a Bunsen lamp becomes crowded with glowing particles.   No glowing particles can be distinguished by the eye or by means of the microscope in a luminous gas-flame, by reason of the rapidity with which the current of gas is carried upwards.   These small particles are stopped in their upward course by any solid body brought into the flame, or by the comparatively still layers of air ; they thus become more compact and dense, and so bring about the deposition upon themselves of further numbers of particles until there is finally formed a visible cloud of soot or smoke. A solid body, or even a layer of air, while stopping the rush of solid particles, simultaneously lowers the temperature of the flame.

In order to render visible the production of masses of solid matter, two blowpipe-nozzles, through each of which a stream of gas issued, were arranged horizontally opposite to one another.   By regulating the distance between the nozzles a perfectly circular homogeneous flame was obtained ; by bringing the nozzles rather nearer to one another and slightly altering their inclination, the flame assumed a half-moon shape.   The lower part of this flame was but slightly luminous, but was filled with little glowing points (fig. 1, Plate II.).   The flame being in a kind of whirling motion, these little points were swept upwards and passed away as sparks from each horn of the half-moon.   By bringing a porcelain plate over these points, the little particles were obtained in the form of coarse-grained soot.   The appearances described become more apparent by causing the gas to issue under diminished pressure; but in this case the regulation of the flow is more difficult.

Another experiment was arranged in which the little particles of carbon were caused to form a larger and visible mass by projection against a solid body.   A platinum basin hung vertically was heated on the concave side by means of a Bunsen's lamp ; a coal-gas flame about 5 centims. in length was caused to issue from the narrow circular orifice of a tube which was held by a clamp, and directed so that the flame struck the basin a little below the centre.   By regulating the distance between this tube and the basin, a halfmoon-shaped flame

was obtained crowded with little glowing particles which ascended with a spiral motion, escaping from each horn of the half-moon (fig. 2). *The separation of solid carbon within a coal-gas flame is thus rendered visible to the eye.* The experiment further shows that in an ordinary coal-gas flame the solid particles are very small but numerous, and that, when a number of these are gathered together in a special part of the flame, that part becomes continuously luminous.

Frankland looks upon the fact of the transparency of luminous flames as militating against the view that they contain solid particles. We know, however, that many substances containing solid matter (for instance, paper soaked in oil) are more or less transparent. Stein has also shown that it is very difficult to distinguish ordinary letters placed behind a gas-flame consisting of several layers, or behind the flame of a petroleum lamp. My own experiments confirm those of Stein, and prove that the lower non-luminous portion of a gas-flame is much more transparent than the upper luminous portion, and that it is almost impossible to distinguish an object when viewed through several layers of such flame, the same object being seen when viewed through a single layer. The eye becomes dazzled by the light of the flame, and so incapable of sharply distinguishing objects which emit lesser degrees of light. Tested in the sunlight, all non-luminous or slightly luminous flames appear exceedingly transparent, luminous flames appear transparent only when viewed in thin layers, and smoky flames only when viewed in small masses.

Frankland asks how a luminous flame can be so transparent as it is if it contain particles of solid carbon. A simple experiment suffices to show that the smoky portion of the flame of turpentine, which Frankland must admit contains *solid* particles, is as transparent as the flame itself. The presence of solid particles but very slightly decreases the transparency of a thick layer of flame. The spaces between the solid particles are comparatively large, and the particles are also in a state of rapid motion ; a continuous image of any body placed behind the flame is therefore communicated to the eye, as is the case when one looks at an object behind a swiftly rotating spoked wheel.

In examining some of these luminous flames in sunlight, it occurred to me that if they really contain solid matter, they should throw a shadow on a white screen placed behind them. The flame of a candle I found experimentally does cast such a shadow as is depicted in fig. 3 ; the shadow extends as far as the luminous flame-mantle. The blue portion of the flame close to the wick, as also the upper blue portion, casts no

shadow ; as the luminous mantle only attains the necessary solidity at some distance above the wick, a space is seen to intervene between the wick and the shadow ; it is only the sides of the lower part of the luminous mantle (through which the sun's rays travel obliquely) that are capable of casting a shadow ; hence the ⌒-shaped form of the lower boundary-line of the shadow. Streaks of varying clearness show themselves throughout the upper parts of the image of the flame ; these are evidently due to the passage of the light through layers of flame of varying densities caused by the differences in temperature of the products of combustion. Fig. 4 represents the shadow thrown by the flame of turpentine ; the soot which appears to the eye in the upper portions of this flame is shown by the shadow to extend continuously almost to the wick.

It seemed to me necessary to test this new method of examining flames by the shadows which they cast in sunlight, by comparing together as many and as various flames as possible.

Having proved to myself that the flame of pure hydrogen casts no shadow upon a white screen, but only the image of a few more or less dense streaks (fig. 5), I caused the gas to traverse an apparatus in which it should become laden with solid material (zinc oxide, magnesia, &c.) before ignition. The flame now in each case cast a most distinct shadow. When finely divided carbon (soot from a turpentine-flame) was thrown into the flame of hydrogen, the flame became strongly luminous, and a shadow at once appeared on the screen.

As an example of a flame undoubtedly containing solid matter, I employed hydrogen laden with the vapour of chromyl dichloride ($CrO_2 Cl_2$). During the combustion of this flame chromic oxide is produced ; this oxide is not converted within the flame into a non-solid substance or substances, as is partly the case with the carbon liberated in the flames of hydrocarbonaceous bodies. Fig. 6 represents the shadow cast by the flame of hydrogen laden with the vapour of chromyl dichloride ; it is very similar to the shadow of the turpentine-flame.

The observation of the shadow thrown on the screen by a flame serves as a means for determining the action which is taking place within that flame. Spontaneously-inflammable silicon hydride throws a very distinct shadow, resembling that cast by a turpentine-flame. The gas is evidently decomposed with the production of solid silicon, which is again oxidized in the outer portions of the flame to silica, itself also a non-volatile solid.

The flame of hydrogen containing the vapour of osmic acid

is found by the shadow test to contain solid matter; this doubtless consists of finely divided osmium.

Kersten states that several metallic chlorides, when brought in the state of solution into the flame of a Bunsen's lamp, cause that flame to become luminous. Those chlorides which thus produce luminosity are, according to Kersten, ferrous, cuprous, palladium, and gold chlorides; chlorides of zinc, antimony, platinum, silver, tin, mercury, as also chlorides of the alkali-metals and alkaline earths, do not, according to the same authority, cause the Bunsen flame to become luminous[*].

These chlorides may cause luminosity by deposition of metal, or by decomposition of hydrocarbons by means of chlorine, and consequent production of carbon within the flame.

In my experiments I substituted the flame of hydrogen or carbon monoxide for that of the Bunsen lamp. I found that the introduction of iron, platinum, and copper chlorides caused these flames to become luminous. The metallic chlorides were used in the solid state, as I found that the few drops of solution which could be introduced into the flame on a platinum wire spirted very much, and contained an extremely small quantity of solid matter. The following Table presents the results of the examination of a series of metallic chlorides and of other salts.

Non-luminous Flames

Became luminous by the introduction of:—		Did not become luminous by the introduction of:—	
Platinum chloride .... Iron chloride .... Chromyl dichloride. ...... Osmic acid .... Ammonium dichromate	Spectra continuous.	Lead chloride, ⎱ (blue-white Lead nitrate ⎰ colour).... Alkali and alkaline-earth salts .... Boric acid (green colour) ..	Spectra line.

Silver nitrate (blue colour).
Stannous and stannic chlo-
  rides (blue colour).......
Manganous chloride(greenish
  colour) ....
Gold chloride (greenish
  colour) ....
Cuprous and cupric chlorides
  (blue and green colours)..
	Continuous and line.	Arsenic and its salts (white-
		  yellow colour)..........
		Antimony and its salts
		  (whitish colour) ....
		Mercuric chloride.
		Magnesium chloride.
		Silver chloride.
			Continuous.

If the substances which are brought into the flame be non-volatile at the temperature of the flame, yellowish-white light is alone emitted, the flame appears very luminous and affords

. [*] In one of Davy's early papers I find an experiment described in which cupric chloride was used to bring about luminosity in a flame.

a continuous spectrum. If the metal or oxide which is separated in the flame be partially volatilized, one part of it may remain in the solid form and so *impart luminosity* to the flame, while another portion may become gaseous and so impart *colour* to the flame. The luminous portions of such flames show a continuous, the coloured portions a line-spectrum.

Those substances which do not cause a non-luminous flame to become luminous may also be subdivided into those (salts of lead, of alkalies, alkaline earths, except magnesia, &c.) which are easily volatilized and therefore colour the flame while affording line-spectra, those (arsenic and antimony compounds) which afford continuous spectra while also imparting a colour to the flame, those (mercuric chloride) which volatilize almost immediately without producing any noticeable effect upon the flame, and, lastly, those (magnesium and silver chlorides) which, although undergoing decomposition with the production of solid matter, nevertheless do not cause luminosity because the solids produced are not carried into the flame. The metal or oxide which is separated in the various experiments may be obtained by bringing a porcelain basin into the flame.

If it be true that the luminosity is due in the foregoing experiments to solid matter separated within the flame while the colour is caused by heated vapours, we should expect to find the luminous flames casting shadows upon a white background when viewed in sunlight, while the coloured flames would cast no shdaows.

Experiment has proved the correctness of this expectation in every case.

The flames of burning magnesium and of coal-gas containing oxygen and metallic zinc both cast very distinct shadows; these flames contain magnesium and zinc oxides respectively, substances which remain solid at high temperatures.

From these experiments I think we may draw the following inference:—

Luminous flames which owe their luminosity to the presence of finely divided solid matter produce characteristic shadows when viewed in sunlight.

But is the converse of this true? Do luminous flames which produce shadows in sunlight owe their luminosity to the presence of solid matter?

To this question, in the absence of experimental evidence, I should answer no; for it is possible that luminous flames consisting only of heated gases may, when viewed in sunlight, cause an appearance similar to that of a true shadow. We know that the electric light, when passed through

burning hydrogen which has been coloured yellow by the presence of sodium chloride, is partly absorbed thereby, and that the light which passes on shows a dark absorption-band. So also sunlight is more or less absorbed when passed through various glowing gases. As sunlight already shows dark lines corresponding with light spectral lines of sodium, barium, calcium, copper, &c., it is not to be expected that flames coloured by these metals in the gaseous state should cause absorption of any part of the sun's rays. But it is otherwise with colourless luminous flames. Such flames absorb portions of the rays of almost every part of the solar spectrum. These flames must therefore cause a shadow-like appearance on the screen, unless the absorption be too trifling to allow of our eye detecting the relatively dark spaces. That the eye is unable to detect any dark spaces, I have proved experimentally. I have not succeeded in obtaining any flame owing its luminosity only to strongly heated gases which is capable of throwing an appreciable shadow on a white screen when viewed in sunlight. That the flames of carbon monoxide, sulphur, selenium, sulphuretted hydrogen, and carbon disulphide should cause no shadow (although affording continuous spectra) may perhaps be chiefly due to the small quantity of light emitted by them. But it was also found that the exceedingly luminous flames of arsenic, phosphorus, and phosphuretted hydrogen burning in oxygen, as also of oxygen and nitric oxide in carbon disulphide, produced no appearance of a shadow on the white screen. The absorption caused by these flames upon the sunlight was therefore too small to admit of detection by the unaided eye. On account of their volatility, arsenious and phosphoric oxides must be present in the gaseous condition in the flames produced by burning arsenic and phosphorus in oxygen. These flames are therefore transparent : it is only at some distance above the flames that the products of combustion assume the solid form ; the white smoke so produced casts a deep shadow on the screen.

From an extended series of observations, we conclude that luminous flames consisting only of gases and vapours are incapable of producing an appreciable shadow when viewed in sunlight ; lighter and darker streaks, due to the varying densities of the vapours, of course appear in the images of these flames thrown on the screen.

The appearance of a shadow is therefore proof of the presence of suspended solid matter in the flame causing the shadow.

It follows from this that the luminous flames of hydrocarbons contain solid matter, inasmuch as they produce very sensible shadows. It is evident that this solid matter can be

nothing else than carbon. The shadow test therefore supplies us with a means of detecting the presence of solid carbon in such flames.

Some time after writing the foregoing pages I noticed a memoir by G. A. Hirn entitled " Sur les propriétés optiques de la flamme des corps en combustion, et sur la température du soleil " *. In this paper Hirn describes the behaviour of various flames when viewed in sunlight ; his conclusions, however, are directly opposed to those which I have deduced.

Hirn starts with the assumption that luminous hydrocarbon-flames contain solid carbon, luminous phosphorus-flame contains solid pentoxide of phosphorus &c.—that the transparency of these flames is due to a change, brought about by the high temperature, in the optical properties of the solid particles contained in the flames, whereby these particles become transparent and incapable of reflecting light.

I shall endeavour to show that Hirn's conclusions are invalid. In his preliminary assumption Hirn takes no notice of Knapp's experiments, which show that the decrease in luminosity of hydrocarbon-flames, brought about by admitting air, is not to be traced to oxidation of the carbon, inasmuch as pure nitrogen causes the same result †. Frankland's supposition that the luminosity of hydrocarbon-flames is not due to the presence of solid carbon had been disputed by no one when Hirn's paper appeared. Frankland had shown, five years previous to the appearance of Hirn's paper, that phosphorus pentoxide is volatilized at temperatures lower than that of the flame of phosphorus burning in oxygen, and that therefore the luminosity of this flame cannot be due to the presence of solid phosphorus pentoxide.

Of Hirn's assumptions—that the luminosity of hydrocarbon-flames is due to the presence of solid carbon, and that the luminosity of phosphorus burning in oxygen is due to the presence of solid phosphorus pentoxide—the first was unproved at the time he wrote, and the second is untrue.

Light reflected from a solid body is known to be polarized. Hirn found the light from ordinary hydrocarbon-flames, as also the light from burning phosphorus, to be non-polarized ; he also failed to detect evidence of polarization in the light coming from the flame of the blast-furnace. He found, however, that the white smoke rising from the phosphorus-flame emitted polarized light, as did also the smoke coming from the blast-furnace when the furnace-doors were opened. He concluded that the flame of the furnace owes its brilliancy to the

* *Ann. Chim. Phys.* [4] vol. xxx. p. 319.
† *Journ. Pract. Chem.* [2] vol. i. p. 425.

presence of those metallic salts which appear in the solid form when the doors are opened. This conclusion tacitly assumes the presence of solid matter in the flame of the blast-furnace. Hirn does not appear to have examined the spectrum of this flame; and, so far as our knowledge extends, the contrary assumption to that made by Hirn appears quite as credible as his.

Hirn thus assumes that the light from a flame which contains solid matter must show evidence of polarization, and that the absence of polarized light is only to be accounted for by supposing that the solid particles become optically altered at a very high temperature, and lose their power of reflecting light. It appears to me that the facts observed by Hirn would be much better explained by regarding the absence of polarization as proof of the absence of solid particles in the flame. This explanation would apply to the phosphorus-flame, and perhaps also to the flame from the blast-furnace. I have, however, convinced myself by experiment that the non-recognition of polarized rays in the light coming from a flame is not proof of the absence of solid matter in that flame.

The amorphous carbon present in the luminous flames of ordinary combustible hydrocarbons reflects almost no light; these flames show no traces of polarized light when examined by means of the polariscope. The light from many other non-homogeneous bodies is also devoid of noticeable polarized rays. We cannot, therefore, assume that the light emitted by a flame containing a small quantity of finely divided solid matter must show such a number of polarized rays as shall be recognizable by the polariscope. Hydrogen-flames, rendered luminous by the presence of platinum chloride, osmic acid, ammonium chromate, ferric and cupric chlorides, &c., showed no traces of polarized light when examined by means of Arago's and Savart's polariscopes; yet these flames certainly contained solid matter.

The flame of hydrogen containing chromyl dichloride also failed to show polarized light: the smoke of this flame, and also of the turpentine-flame, when examined in sunlight, afforded evidence of polarization. I am inclined to trace the polarizing action of the smoke in these cases to the presence of condensed water, tarry matter, hydrochloric acid, &c., which substances would be present as gases in the flame itself.

I cannot, therefore, regard the non-detection of polarized light as proof of the absence of solid matter in flame; much less can I agree with Hirn's statement, that "the solid particles lose their power of reflecting light at a white heat."

In the second section of his paper Hirn notices a statement made by Offret*, who, reasoning from Arago's observation that the luminous effect of a flat gas-flame, as measured by the photometer, is the same whether the broad or narrow end be turned towards the instrument, concludes that the luminous gas-flame is *completely transparent*. Hirn shows from his own observations that Arago's statement is not quite correct, and that the broad side of the flame always emits a little more light (about one fifth more) than the narrow side of the flame. Offret mentions the well-known fact that the light from an oil-lamp or candle, when thrown through the electric or lime light, casts a shadow on the opposite wall : he, and Hirn also, regards this phenomenon as due to refraction caused by the heated, and therefore thin, layers of gas.

I have, however, shown that luminous flames throw true shadows when carefully examined, and that these shadows are independent of the darker and lighter bands caused by refraction.

In section 3 of his paper Hirn examines the transparency of the flame of petroleum-lamps for light emitted from flames of the same kind. An old-fashioned shadow photometer was employed. A system of eight large lamps placed one behind another, served as the source of light. Hirn's observations and calculations led him to the conclusion that the light from such lamps suffers a greater diminution by passing through the hot gaseous products of combustion, than by passing through the luminous flames themselves. The method of calculation adopted by Hirn appears to me to be altogether erroneous. By applying what I must regard as a more rational method of interpretation to Hirn's results, I find that there is a decided decrease in luminosity brought about by the action of the separated carbon upon the light passing through the flames of these lamps, over and above that occasioned by refraction in passing through the heated layers of gas†.

Hirn discusses the transparency of flames for light from foreign sources in the fourth section of his paper : he again unfortunately chooses the flame of burning phosphorus. From the fact that the flame of burning phosphorus causes no shadow, while the smoke, so soon as it becomes visible to the eye, does cast a shadow, Hirn concludes that the solid phosphorus pentoxide suspended in the flame is transparent ; we

* Essay presented to the Société d'Agriculture, Douai.

† [The original paper contains Hirn's numbers, with a description of his, and of Heumann's method of calculation : the latter method is certainly much the more rational. Hirn's photometric process is also shown by his own results to be very faulty.—M. M. P. M.]

have long known, however, that the oxide does not exist in the solid form in the flame.

I have noticed a faint shadow cast by the flame of phosphorus burning in air ; but as this shadow disappears on substituting oxygen for air, I conclude that in the former combustion the temperature is scarcely high enough to ensure the complete conversion of solid into gaseous matter.

Hirn further says that the flame of a petroleum-lamp throws no shadow on a screen when viewed in sunlight, but that when the flame is rendered smoky the smoke causes a most marked shadow. From the detailed description of Hirn's experiment, it is evident to me that he placed the screen at too great a distance from the flame. He also obtained no distinct shadow when the sunlight was sent, by means of a looking-glass, through a series of eight petroleum-flames. On account of the interference between the flames, and also on account of their distance from the screen, this result is not to be wondered at [*].

The petroleum-flame employed by Hirn was surrounded by a glass cylinder, which seriously interfered with the distinctness of the shadow produced.

It is not possible, by removing the screen further and further from the flame, to obtain a clearly defined shadow much exceeding in size the dimensions of the flame itself. If, however, the sunlight be collected by a convex lens, and be thrown on the flame, a large and well-defined shadow may be obtained ; the eye is also less fatigued by the glare of the white screen.

Fig. 7 represents the shadow thrown by a non-smoky coal-gas flame, burning at the orifice of a tube 7 millims. in width : fig. 8 represents the appearance on the screen after removing the luminosity of this flame by admission of carbon dioxide. If the flame was rendered wholly or partially non-luminous by the introduction of a piece of metal, the shadow wholly or partially disappeared : emission of light and production of shadow are therefore closely related to one another.

The flat flame of a *very small* bat's-wing burner produces no shadow ; but if the flame be turned edgeways towards the screen, a small but distinctly perceptible shadow is produced. In the first instance the layer of luminous flame is too thin to cause the production of a shadow ; if an ordinary-sized burner

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[*] Hirn remarks that the smoke arising from a petroleum-flame appeared white when illuminated in a dark room. This is, I think, due to reflection of light from the smoke, chiefly from the particles of water, tarry matter, &c., the black smoke being itself nearly invisible against the dark background.

be employed a shadow is obtained.   This shadow is most distinct towards the outer edges of the flame, where the thickness of luminous matter attains a maximum.

Fig. 9 represents the shadow thrown by the flame of an ordinary burner.

Fig. 10 represents the shadow produced by directing the small flame against the screen.   Figs. 11 and 12 show the forms of shadows produced by adding an excess of benzol vapour to the gas (coal-gas or hydrogen) issuing from ordinary burners.   Similar appearances result with the flame of hydrogen rendered luminous by admission of chromyl dichloride.   These experiments show that the shadows increase in intensity as the thickness of the luminous layer increases, or, in other words, that the intensity of the shadow is dependent upon the number of carbon particles which prevent the passage of the sun's rays through the flame.

Hirn blew lycopodium powder into a flame ; before the powder was completely ignited the flame produced a shadow ; after complete ignition, however, no true shadow was obtained. I have carefully repeated this experiment, and find that a flame containing a considerable quantity of lycopodium powder produces a *marked shadow*.   Hirn probably used too little of the powder, or placed his screen at too great a distance from the flame.   It is of course to be expected that the shadow produced by a flame in which the lycopodium powder is undergoing thorough combustion should be less marked than that formed by a flame which contains unburned lycopodium : in the former case the shadow is due to the presence of separated carbon only.

The sole experiment of Hirn which is capable of interpretation in terms of his theory only, is that in which the light of burning magnesium wire was passed through the flames of eight petroleum-lamps : the increase in the light-effect of these eight flames was equal to the total light-effect of the magnesium when burned by itself.   But Hirn's previous experiments showed that the light from two petroleum-lamps loses 39·9 per cent.* when passed through *six* other similar flames ; it is scarcely possible therefore that the light of burning magnesium could suffer *no* diminution in luminosity when passed through *eight* petroleum-flames.   Hirn's photometric process is, as his own results show, altogether untrustworthy.

Hirn says that the flame of the blast-furnace is completely transparent : this may be so.   His conclusion, that this flame contains solid matter, but that this solid matter is optically

* Calculated according to my method.

changed at the high temperature of the flame, is, I think, based upon no solid foundation of fact.

Hirn describes experiments with coloured flames (Bengal fire, &c.). His conclusion, that these flames contain much solid matter, although they cast no shadow, is evidently incorrect. The colour is, of course, due to vapour, not to solid matter.

Lastly, Hirn regards the petroleum-flame as diathermanous; but even admitting that this flame allows more or less the passage through it of heat-rays (a problem which I am not in a condition to examine experimentally), this would not contradict the *demonstrated fact* of the presence of solid carbon in ordinary luminous flames.

I think I have now shown that Hirn's hypothesis, viz. that solid bodies when raised to a white heat lose their power of reflecting light and become transparent, is without experimental support, and therefore cannot be maintained.

In the attempt to discover the causes of the luminosity of flame, it is necessary that the observer should be acquainted with the physical as well as with the chemical data which he will be required to examine. A want of this twofold knowledge has already led many to view the phenomena of luminous flames either from the purely physical, or from the purely chemical standpoint; in either case the result has been disastrous.

In conclusion, I would once more draw attention to the observations of W. Stein. If soot be present in the form of vapour in luminous flames, the application of a high temperature, after condensation, should cause it to again assume the gaseous form; but this is not the case. Further, the soot deposited from a coal-gas flame does not contain more than 0·9 per cent. of hydrogen.

I would now gather together the

Proofs of the Presence of Solid Carbon in Luminous Hydro-carbon-Flames.

(1) *Chlorine causes an increase in the luminosity of feebly-luminous, or non-luminous, hydrocarbon-flames.* Inasmuch as chlorine decomposes hydrocarbons at a red heat, with separation of carbon, it follows that the increased luminosity is due to the production of solid carbon particles.

(2) *A small rod held in the luminous flame becomes rapidly covered on its lower surface (the surface opposed to the issuing gas) with a deposit of soot.* The solid soot is evidently driven against the rod. If the soot were present as vapour in the luminous flame, its deposition would be due to a lowering of

the flame-temperature, and would therefore take place on all sides of the rod.

(3) *A strongly heated surface also becomes covered with a deposit of soot.* This would not be possible if the deposit were the result of the cooling action of the surface upon the flame.

(4) *The carbon particles present in the luminous flame become visible when the flame is caused to rush against another flame or against a heated surface.* The separated particles are rolled together into larger masses, *so that the luminous mantle becomes filled with numerous glowing points.* The soot of such a flame is very coarse-grained.

(5) The luminous mantle of a flame is not altogether transparent : the thicker the flame-layer and the greater the number of solid particles contained therein, the less transparent does it become. *The transparency of a luminous flame is no greater than that of the (approximately) equally thick stratum of soot which rises from the flame of burning turpentine, and which is universally allowed to contain many solid carbon particles.* The luminous flame of hydrogen, containing solid chromic oxide, is as transparent as the hydrocarbon-flame.

(6) *Those flames which undoubtedly owe their luminosity to the presence of finely divided solid matter, produce characteristic shadows when viewed in sunlight.* The only luminous flames which do not produce true shadows are those which consist of glowing vapours and gases. *Luminous hydrocarbon-flames produce strongly marked shadows in sunlight; these flames therefore contain finely divided solid matter.* That this solid matter can be nothing but carbon is evident from the fact that other substances, capable of remaining solid at the temperature of these flames, are absent.

These proofs are, I think, sufficient to convince every one that the luminous flames of hydrocarbons actually contain solid carbon particles.

Darmstadt, Chemisches Laboratorium des
    Polytechnicums.

LI. *On a Method of determining the Amount of Proteine Compounds in Vegetable Substances.* By J. ALFRED WANK-LYN, *Corresponding Member of the Royal Bavarian Academy of Sciences, and* W. J. COOPER [*].

THE physiological doctrine that the animal does not produce proteine compounds, but simply transforms those proteine substances which it has taken in as food, lends great

[*] Communicated by the Authors.

importance to the determination of the amount of proteine compounds in different kinds of vegetable food ; and such a determination becomes of the utmost importance both to the physiologist and from a practical point of view.

Hitherto, however, this desideratum has been very imperfectly supplied, and the chemist has very inadequately answered the question as to the proteine value of the different vegetable foods. Gluten, legumen, vegetable caseine, vegetable albumen, as the various proteine substances occurring in vegetables have been called, vary much in properties. Some of them are soluble and others are insoluble in water ; and some are soluble in alcohol ; and it would be difficult to draw up any general method of extracting the proteine compounds from vegetables so as to be enabled to weigh the proteine compound in a state of purity. Resort has therefore been had to elementary analysis ; and chemists have deduced the amount of proteine compounds from the percentage of nitrogen found on submitting the food to ultimate analysis.

To this procedure there are several objections which have, apparently, not been sufficiently insisted upon. Taking the case of wheaten flour (which is much more favourable than many other cases), the percentage of nitrogen is a little short of 2·00 ; yet neither the Will-and-Varrentrapp process nor the Dumas process of nitrogen-determination, as it is generally carried out, is at all adequate to the valuation of the proteine substance in flour.

The Will-and-Varrentrapp process, as those who have a critical knowledge of it are aware, is subject to special failure when it is applied to proteine substances, and is not a determination of nitrogen in these instances.

The Dumas method, as usually practised, is uncertain when it is applied to determine a minute quantity of nitrogenous substance in presence of a large quantity of non-nitrogenous organic matter. Possibly, if carried out with extraordinary care and extraordinary precautions, the Dumas process might become available for the purpose in view ; but those persons who have practical knowledge of the difficulties besetting this particular case will admit that extraordinary care would indeed be required, and that the process would be too impracticable for general employment.

The method by which we seek to accomplish the task before us is, we believe, especially adapted for this description of work.

We propose to measure the amount of proteine substances in vegetables by the amount of ammonia which the vegetables *generate* when they are subjected to the action of a boiling solution of potash and permanganate of potash ; in fact, we

have made a special adaptation of the well-known ammonia process of water-analysis to the case of vegetable proteine.

The working details of our process are as follows :—

Into a litre flask a carefully weighed gramme of the vegetable substance to be analyzed is placed, and 20 cub. centims. of decinormal solution of caustic potash is added, and then water is added until the litre-mark is reached by the level of the liquid. The contents of the flask are then shaken up so as to ensure thorough mixture. In this manner we obtain a liquid of such a strength that each cubic centim. contains 1 milligramme of the flour or other vegetable substance to be operated upon. 10 or 20 cub. centims. of this liquid (*i. e.* 10 or 20 milligrammes of the vegetable substance) are convenient quantities to work with.

The next step is to get the retort in order as for a water-analysis, and to place in it 300 or 500 cub. centims. of good drinking-water, and to add 50 cub. centims. of a solution containing 10 grms. of potash and 0·4 grm. of permanganate of potash (such as is used in water-analysis), and to distil until the residue in the retort no longer yields the slightest trace of ammonia. That having been done, 10 or 20 cub. centims. of the liquid containing the vegetable substance are to be added and the distillation proceeded with. The vegetable substance will then be attacked, and its proteine will yield ammonia, which will distil over and may be measured by means of the Nessler tests. For further details of the manner of carrying out work of this description we would refer to the Treatise on Water-analysis, which is now sufficiently well known to chemists.

It was shown some years ago that egg-albumen yields about one tenth of its weight of ammonia when submitted to such a process as the above, and that solutions containing different quantities of egg-albumen yield ammonia exactly proportional in amount to the strength of the solutions of albumen. Our experiments warrant a parallel statement in the case of vegetable proteine ; and in the Table about to be given, the ammonia, multiplied by 10, gives a fair approximation to the actual quantity of vegetable proteine in the different samples.

As will be observed, our experiments include many descriptions of wheaten, pea-, rice-, maize-flour, oats, barley, malt, rye, and arrowroot. The last-named is important as showing a very small proportion of proteine.

The pea-flour was ground from the peas in our own laboratory, and passed through a very fine sieve. The rice-flour was likewise of home-manufacture ; and the same is true of maize and the malt. The rest were not powdered in the laboratory.

Samples of wheaten flour :—

	Name of sample.	Percentage of ammonia.
1.	Cambridgeshire extra-superfine...	1·10
2.	Another sample ,, ...	1·00
3.	Household flour, Waterloo Bridge.	1·13
4.	Country flour	1·08
5.	Huntingdonshire	1·05
6.	Suffolk...............................	1·00
7.	Hungarian	1·10
8.	Another Hungarian	1·05
9.	,, ,, ...	1·07
10.	Darblay, Paris	1·05
11.	Vienna................................	1·08
12.	Australian	0·92
13.	Californian	1·13
14.	American	1·14
15.	Another American	1·17
16.	,, ,, ...	1·09
	Pea-flour...........................	2·30
	Rice ,, ...	0·62
	Maize ,, ...	1·03
	Oats.................................	1·00
	Barley	1·10
	Malt	0·50
	Rye	1·45
	Arrowroot	0·08

In looking through this Table the reader will be struck with the constancy of the quantity of proteine substances in wheaten flour. If one of the American samples (No. 15) be excluded (we believe that there was a little pea-flour in that sample), it will be seen that the highest percentage of ammonia given by any sample of flour is 1·14; and excluding the solitary sample of Australian flour, the lowest yield of ammonia is 1·00. Wheaten flour would therefore seem to yield between 1·00 and 1·13 per cent. of its weight of ammonia when subjected to the above process, and, according to the rule above given, would seem to contain from 10·0 to 11·3 per cent. of proteine compounds.

Maize, oats, and barley, as will be seen, very closely resemble wheaten flour in yield of albuminoid ammonia. Rye, on the other hand, is exceptionally rich in proteine. Apparently it is the most nitrogenous cereal.

The high percentage of ammonia from pea-flour will attract attention; and, applying the rule, pea-flour contains 23·0 per cent. of proteine, which accords with what is known of the pea. The proteine in rice amounts to about half as much as in wheaten flour.

LII. *Notices respecting New Books.*

The Colorado-Plateau Region considered as a Field for Geological Study. *By* G. K. GILBERT. 8vo pamphlet. Tuttle and Co.: New-haven, Conn. 1876.

IF we proceed from the Mississippi valley across the plains to the westward, there are at first but few points of especial interest to the geologist.

The wide undulating plain-land, though necessarily elevated above the sea-level, has suffered little in its upheaval; and the strata, though bent and wavy, present none of those great alterations of structure and position that characterize the mountainous backbone which borders the western edge of the plains and extends to the Pacific Ocean.

Here violent destructive upturnings of strata, complicated folds and flexures, faults of great magnitude, and all the concomitant circumstances of strata crushed, upheaved, and broken, give opportunity for study to the scientist and wondrous scenery for the artist.

But within this region lie a series of plateaus or high plains, which, though seamed and fissured, altered and faulted, have experienced less violent alterations than its neighbouring ranges. To this has been given the name of the Colorado-Plateau Province, from the mighty river which, passing through it, has cut its way through the successive layers, and formed, with its tributaries, a group of formidable gorges or cañons, the origin of which it is the purpose of this memoir briefly to elucidate.

These plateaus are from 5000 to 11,000 feet above the sea-level, terminated generally by high cliffs both in the cañons and elsewhere; and the extreme dryness of the climate renders the vegetation so scanty that the rock-masses are fully exposed to the research of the scientific examiner. "There is no need to search for exposure where every thing is exposed." Dr. Newberry, speaking of one of the Southern Plateaus, says, "On our way to the Moqui villages we passed through a region singularly favourable for accurate geological investigation; where there is no vegetation to impede the view; where the strata are entirely undisturbed, and are cut by valleys of erosion in the wall-like sides of which every inch of the series may be examined.

"In this journey we ascended in the geological scale from the summit of the Carboniferous to the base of the Cretaceous Series. Of this interval there is no portion of which the exposures are not as complete as could be desired." (Geol. Ives's Exped. p. 77.)

The most arid, and at the same time obscure, portions are the valleys between the ranges, "which are filled with quaternary gravels and clays which hide all other beds;" but in the great river-channel the water has worn so vertically through the strata that there is a full exposure of the material, the only drawback being the narrowness and consequent darkness of many of these

strange gorges. Thus the Colorado itself emerges from the plateau region in a cañon 4000 feet deep.

The chief economic product of the area is coal; and though the amount of glacial drift is inconsiderable, at least a tenth part of the sedimentary rocks of the region is buried beneath vast fields of lava.

As before remarked, the disturbance in the Colorado district can scarcely be considered violent. The faults and folds are numerous, but simple in character; and the "monoclinal fold," or one single bend in the strata, sometimes accompanied by faulting at one extremity of the long ridge or slope formed by the disturbance, is not merely "a feature of occasional occurrence," but is "a characteristic type of displacement, and is rivalled in frequency only by the fault." These monoclinal folds divide the area into a series of "blocks," and sometimes culminate in lines of displacement. A very clear sketch showing the occurrence of these, as well as of the cañons which seam the plains between them, from Major Powell's report on the Exploration of the Colorado, is given to illustrate the region.

But it is in the concluding section of the memoir that its value chiefly lies: in fact the introductory matter is chiefly designed to lead up to the theory of how the cañons of the region were formed; and this is argued out with great completeness.

Their cause is entirely erosion, the natural processes of which may be called disintegration and transportation—the former effected by "weathering" and by the action of running water (for which the term "corrasion" is used), the latter by the action of the streams which carry away the disintegrated material to deposit it at a lower level.

The chief agents of weathering are solution by water, which dissolves the cementing material, splits the rock-mass when frozen in fissures, occasioned by changes of temperature, or, as rain, beats against and destroys the feeble coherence of earths. Plants, too, lend their aid by increasing the power of the percolating water. In corrasion, solution and mechanical wear are the active agents; "the effective force is that of the current; the tools are mud, sand, and boulders;" in the latter case, clear pure water "corrades their beds" by its solvent powers only.

Both these causes have tended to the formation of the cañons; but corrasion has outstripped weathering in its marked effects.

But the rapidity of erosion depends again on declivity, character of rock, and climate, though the former does not affect the dissolving power of water as it does its corrading power; so that it may be fairly assumed that "a deep declivity favours transportation, and thereby favours corrasion;" and "the rapid, but partial, transportation of weathered rock accelerates weathering," by admitting air, frost, and damp, though still the complete removal of the products retards weathering.

It stands to reason, too, that "erosion is most rapid when the eroded rock offers least resistance;" and here the nature of the material is important. The solubility of the rock is increased

2 C 2

when the cement is soluble and the texture of the other ingredients open ; " disintegration by frost is most rapid in rocks which absorb a large percentage of water," detrition by mechanical wear in soft rocks ; and transportation is easiest when the results of corrasion are finely comminuted.

As a rule, climatal effects depend rather on rainfall or dampness, than on changes of temperature, as the latter must be extreme to produce much useful fissuring, though still cracks in a country where frost has an opportunity of acting would facilitate the destruction of the mass. One of the most valuable results of rainfall is the increase the vegetable products afford to the solvent power of water ; but nevertheless, on the other hand, vegetation acts as a protective agent to the surface, both in checking the force of the rain-drops and in retarding the removal of the soil by its rootlets and fallen leaves. Thus in arid regions of sufficient steepness " absence of vegetation is accompanied by absence of soil," the rain runs off the rock-surface, and both solution and weathering are therefore retarded, though the disintegration by corrasion would in many cases be increased.

From these data the author arrives at the following conclusions, in a very instructive summary.

That " a stream of water flowing down its bed expends an amount of energy that is measured by the quantity of water and the vertical distance through which it flows ;" and its capacity for transportation is enhanced by comminution, according to the fineness of the detritus on which some portion of the stream's energy must be expended. Thus a stream with a maximum load of fine material will be slower than when carrying a maximum load of coarse débris ; and the capacity of a stream for transportation is greater for fine débris than coarse.

Still a river cannot do its full work with too full a load, as it is then on the verge of deposition ; nor can it do as much work with small or soft débris as with stouter materials, as the blows dealt with the fragments on the river-bottom are the more effective as the current is strong, the bottom soft, and the fragments hard.

Hence it follows that in the Colorado region the erosion that commenced with the first lifting above the ocean has progressed continuously. In the uplands which border the cañons the rainfall is small and the declivity great, so that weathering is less favoured than transportation ; but in the gorges themselves corrasion becomes more important, owing to the " quantity of water, which belongs to the mountain sources of the stream and not to the plateau which they divide ;" while the great declivity of the bed, coupled with a moderate supply of débris " sufficient for the work of excavation, but not so great as to consume the energy of the current," are important factors in calculating the amount of work done. If the amount of loose material were such as to cause the energy of the stream to be used up in transportation, the result of a river's action would simply be to generally degrade the plain ; but where weathering is not rapid and the stream is well supplied with

water, with a fair amount of detritus and a swift current, the results of corrasion will be proportionately rapid, and gorges, such as those of Colorado, the inevitable result. Then, again, the evenness of the river-bed, even when cut through rocks of various degrees of hardness, depends on the relation of corrasion to transportation. There are no waterfalls in the great cañons, though hard bands of limestone as well as adjacent beds of soft sandstone are equally cut through. Ordinarily this would result in the production of ledges and waterfalls ; but there are none such in these river-beds. The even volume of water from a distant source, the regular velocity of the current, the nature and amount of the débris, lead rather to an equalization of the channel than to its abrupt change. " A stream which has a supply of débris equal to its capacity, tends to build up the gentler slopes and cut away the steeper." With less material and an equally rapid current, the wear would be unequal; but evenness of corrasion is the natural outcome of a state of things similar to that which characterizes the great rivers of Colorado.

In fact it may be said that the cause of the gorge of the Colorado region is due, first, to the general dryness of the plateau, and consequent diminished weathering, while still into the area, and through the probably original fissures, large volumes of water, procured from a distant source, roll down a steep declivity, laden with hard " tools " of stone ; and the regularity of the current has tended to the steady continuous wearing-down of the rocks, unaided by much local weathering, which, had it been present, would have sooner or later converted the precipitous sides of the great cañons into wide river-valleys.

The pamphlet deals little with any general description of the terrain. It is in its entirety an argument in favour of the view that the great cañons are the simple result of ceaseless even water-action ; and, as such, it is not only most interesting and instructive, but most readable.

LIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 311.]

June 15, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

THE following paper was read :—
" Experiments on Contact Electricity between Non-Conductors." By Joseph Thomson, Student at the Physical Laboratory of Owens College.

It was observed that when a plate of copper was lifted from a plate of glass the copper was electrified, and also that when a plate of glass was lifted from a plate of wax the glass was electrified, care being taken to have as little friction as possible ; it was afterwards found that the former experiment had already been made by Fechner (see Wiedemann's ' Galvanismus,' page 21), who also tried lifting copper from sulphur and got the same effect ; although the

plates were lifted as carefully as possible, yet it was not certain that friction had been entirely got rid of, so the following experiments were made to show that there is an electrical displacement when two non-conductors or a conductor and a non-conductor are put in contact without friction.

The arrangement used was as follows :—

Glass rods, AB, CD, EF, GH, were fixed in a wooden frame ACGE; round these rods silk threads, BF, DH, were wound; an aluminium needle carrying a mirror, M, was hung by a silk thread from a brass rod, T, fastened in the wooden frame; a wire from the needle dipped into a glass vessel, N, containing sulphuric acid; a small magnet was fastened to the back of the mirror, and a glass case was placed over the whole; outside the glass case were magnets, by means of which the position of the needle was regulated; a wire also from the outside dipped into the vessel N, and was used to charge the needle with electricity; positive electricity was got from an ordinary electrophorus, negative from an electrophorus in which the resin was replaced by a plate of glass; which was excited by silk. If wax and glass were the substances experimented on, a cake, OQRP, was made, one half of which, OSQ, was glass, the other half, RPS, being wax; the junction of the wax and glass was parallel to OQ, the wax sticking fast to the glass : this cake was then placed on the silk threads under the
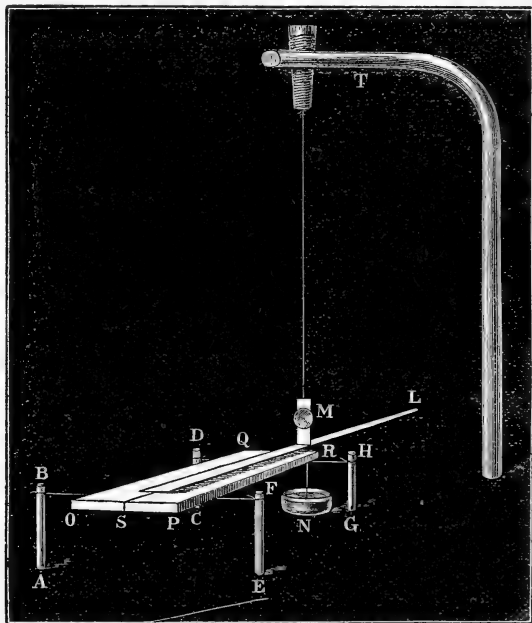
needle, and it was found possible to bring the needle into such a position that when it was charged with positive electricity it was

deflected from the glass part of the cake, when charged with nega-
tive it was attracted towards it. In order to get rid of any electricity
which might have got on the cake in the making, the cake was
made the day before it was placed on the threads, and the experi-
ment was made at least a day, sometimes a week, after putting
the cake on the threads ; pieces of glass and sulphur which had
been treated in as nearly as possible the same way as those of which
the cakes were made were taken and placed separately on the
threads, but no electricity could be detected on them.

The following cakes were tried :—

Glass and wax .... glass +, wax − ;
Glass and resin .... glass +, resin − ;
Glass and sulphur ...... glass +, sulphur − ;
Glass and solid paraffin .. glass +, paraffin − ;
Zinc and sulphur ...... zinc +, sulphur − ;
Sulphur and vulcanite .. vulcanite +, sulphur − ;

the series so far being in the same order as the frictional series ;
this seems to suggest that the electrical displacement which takes
place when two non-conductors are put in contact acts as a pre-
disposing cause, in virtue of which the work done by rubbing them
together is converted into electrical separation.

The following experiment, suggested by Dr. Schuster, was also
tried : the needle was taken out of a Thomson's quadrant elec-
trometer and replaced by a needle, half of which was sealing-wax
and the other half glass ; the quadrants C and D were connected
with the earth ; and it was found possible, after the needle had
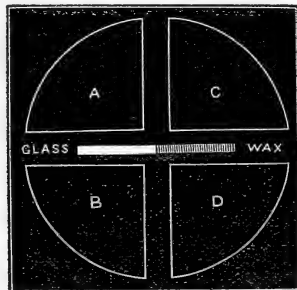
stood undisturbed for ten days, to set the needle in such a posi-
tion that when the quadrants A, B were charged with negative
and positive electricity respectively, the needle rotated in the di-
rection of the hands of a watch, and when A and B were charged
with positive and negative electricity respectively, the needle ro-
tated in the opposite direction.
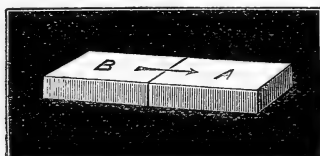
When two non-conductors, A, B, are placed in contact, the electricity will not be distributed through the whole of A and B; and if the displacement is from B to A and parallel to the sides of the cake, the only parts electrified will be the middle and ends of the cake; the A end of the cake will be positively and the B end negatively electrified; and if A has a greater specific inductive capacity than B the middle will be negatively electrified; if it has a less specific inductive capacity than B it will be positively electrified.

I intend to examine more substances, and to endeavour to make some quantitative measurements. The above experiments were made in the Physical Laboratory of Owens College, Manchester; and I have much pleasure in thanking Dr. Stewart, Dr. Schuster, and Mr. Kingdon for the assistance they have given me.

GEOLOGICAL SOCIETY.

[Continued from p. 313.]

March 21st, 1877.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read :—

1. " On the Strata and their Fossil Contents between the Borrowdale Series of the North of England and the Coniston Flags." By Prof. Robert Harkness, F.R.S., F.G.S., Professor of Geology in Queen's College, Cork, and H. Alleyne Nicholson, M.D., D.Sc., F.R.S.E., F.G.S., Professor of Natural History in the University of St. Andrews.

The object of this paper was the investigation of the strata between the great volcanic series of the Lake-district, the Borrowdale rocks, and the sedimentary rocks called Coniston Flags by Prof. Sedgwick. The Borrowdale series, the Green Slates and Porphyries of Sedgwick, are underlain by the Skiddaw Slates, forming the base of the Silurian series, and equivalent in age to the Arenig rocks of Wales, according to their fossil contents. The Borrowdale rocks consist of ashes and breccias, alternating with ancient lavas, and are partly subaërial, partly submarine. They contain no fossils, except in a band of calcareous ashes near the summit of the group, which is followed by the Coniston Limestone, with or without the intervention of a bed of trap. The fossils are of Bala types. Sometimes this band is recognizable with no traces of fossils except cavities filled with peroxide of iron. The authors regard this as proving the prevalence of volcanic activity in the Lake district up to the later portion of the Bala period.

The deposits specially discussed in the paper sent, lie, apparently quite conformably, upon the Borrowdale rocks, and are grouped by the authors as follows, in ascending order :—

        1. Dufton Shales.
        2. Coniston Limestones and Shales.
        3. Graptolitic Mudstones or Skelgill Beds.
        4. Knock Beds.

The " Dufton Shales " are a well-marked, but locally distributed group of muddy deposits, especially well developed in the Silurian area underlying the Cross-Fell range, where they are seen in four principal exposures ; and their thickness probably exceeds 300 feet. They are richly fossiliferous, the fossils being generally of Bala types ; and they may be regarded as forming, palæontologically, the base of the Coniston Limestone. The fossils sometimes occur in ash-beds ; and the continuance of these conditions leads the authors to believe that there was no break between these shales and the underlying Borrowdale rocks.

The " Coniston Limestone " has long been recognized as the best-defined division of the Lower Silurian rocks of the north of England. Its range and characters, and those of the associated shales in different localities, are indicated by the authors ; and from the contained fossils, they refer it, at least approximately, to the horizon of the Welsh Bala Limestone, whilst they regard it as the precise equivalent of the Lower Silurian of Portraine (co. Dublin), and of that of the Chair of Kildare, both of which are of Bala age.

The " Graptolitic Mudstones " overlie the Coniston Limestone wherever the summit of the latter is to be seen.   Besides Graptolites, they contain many other fossils, including Corals, Brachiopods, Cephalopods, and Crustaceans ; and from the consideration of the whole fauna, the authors are led to believe that the position of these deposits must correspond either with the highest beds of the Bala series, or with the lower portion of the Llandovery group.   In their opinion, there is perfect conformity between the Mudstones and the underlying Coniston Limestone.   They regard the Graptolitic Mudstones as constituting a definite geological horizon of more than local importance, as they have been recognized in Ireland, Sweden, Carinthia, and Bohemia.

The Graptolitic Mudstones are succeeded by the " Knock beds," so called from their great development in Swindale Beck, near Knock.   Wherever they occur they consist chiefly of pale green fine-grained slates, very ashy in appearance, and presenting many dendrites, and frequently crystals of cubic pyrites.   There is no evidence of unconformity between them and the underlying Mudstones.   The former contain scarcely any fossils.   They are directly surmounted by the " Coniston Flags," representing the Denbighshire Flags of North Wales, which have been shown to be of Upper Silurian age.   Hence the authors conclude that the Knock beds must be either the basement series of the Upper, or the summit series of the Lower Silurian, or else a group of passage-beds between the two.   The palæontological evidence is insufficient to settle the point ; but it tends to show that the Knock beds are at the base of the Upper Silurian—an opinion which is corroborated by their lithological resemblance to the Tarannon slates of Wales.

The paper concluded with an appendix on the Irish rocks referred to above.

2. " On a new Area of Upper Cambrian Rocks in South Shrop-

shire, with the Description of a new Fauna." By C. Callaway, Esq., M.A., F.G.S.

The purpose of the author was to prove that certain olive, mica-ceous, thin-bedded shales exposed at Shineton, near Cressage, and covering an area eight miles in length by two in the greatest breadth, which had been mapped as Caradoc in the survey, were of Tremadoc age. They were seen clearly to underlie the Hoar-Edge Grit, the lowest beds in the district, with Caradoc fossils; and no rock distinctly underlying the shales could be detected. The evidence for their age was chiefly palæontological. With the exception of *Asaphus Homfrayi*, a Tremadoc form, the species are new. Genera such as *Olenus, Conocoryphe, Obolella*, and *Lingulella* suggested a very low horizon; but two Asaphoid forms (though not typical *Asaphi*) pointed in an opposite direction. Corroborative evidence was found in a correlation of the shales at Shineton with the *Dictyonema*-shales at Pedwardine and Malvern. It was shown from lithological characters and from fossils that the shales at the three localities were of the same age; and as the beds at Pedwardine and Malvern were, on their own testimony, admitted to be of *Lingula*-flag or Tremadoc age, the Shineton shales were inferred to be on the same horizon, the Asaphids leading the author to adopt the younger of the two formations. He was of opinion that the Black Shales of Malvern (Dolgelly beds) were not represented in the Shineton area. He announced the discovery of the Hollybush Sandstone, forming a continuous band between the Shineton Shales and the Wrekin axis, recognized by the occurrence of *Kutorgina cingulata*, and probably separated from the shales by a fault. This also afforded corroborative evidence of the identity of the *Dictyonema*-shales with the shales at Shineton.

April 11th, 1877.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read :—

. 1. " On Sandworn Stones from New Zealand." By John D. Enys, Esq., F.G.S.

The author exhibited specimens of sandworn pebbles from near Wellington in New Zealand, and described their mode of occurrence. They are found on an isthmus rising but little above the sea, and about a mile wide, and having on each side a line of low sandhills, separated by a flat space of clayey sand, on which the stones rest. The isthmus separates two bays, on each side of which the ground is high; and hence the prevailing winds (which are north-west and south-east) blow across the isthmus with considerable force, and carry with them a cloud of sand, which, on a windy day, forms a dense mass reaching about to the knees of a person walking over the ground. The passage of this moving sand over the stones or pebbles lying on the surface wears them away so as to give them sloping sides, and even to bring them to an angle or ridge running along the upper surface, the direction of the longer axis of the stone with respect to the prevailing wind governing the par-

ticular form assumed by the worn stone. Where veins of harder
material occur in the stones, these are left projecting from the
surface, and are sometimes even undercut.

2. " The Bone-caves of Creswell Crags."—Third Paper. By the
Rev. J. Magens Mello, M.A., F.G.S.

3. " On the Mammal-fauna of the Caves of Creswell Crags." By
Prof. W. Boyd Dawkins, M.A., F.R.S., F.G.S.

LIV. *Intelligence and Miscellaneous Articles.*

SUPPLEMENT TO A THEORY OF THE CUP-SHAPED RADIOMETER.
BY PROFESSOR CHALLIS, M.A., F.R.S.

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

I BEG to be allowed to make a few supplementary remarks re-
lative to the Theory of the action of the cup-shaped radiometer
which I proposed in the April Number (p. 278).   On further con-
sideration of the problem, it seems to me that this form of the
experiment is specially adapted to reveal the character of the forces
concerned in producing the observed movements.   In that theory
it is assumed that the radiant light or heat incident on the vanes
is converted into heat of temperature, whereby the atoms in a very
thin superficial stratum are displaced from their neutral positions.
The only particular relative to this view that I have occasion to
advert to is the assertion (in p. 280) that " the state of displace-
ment of the atoms induced by a disturbance on one surface is
spread equally over both by superficial conduction."   It now appears
to me that since the light is equally incident on all parts both of
the convex and the concave bright surfaces, there is no reason to
attribute to the accession of temperature any other effect than an
expansive force acting in directions *normal* to the surface, and
causing the superficial atoms of the concave surface to be *more*
crowded, and those of the convex surface to be *less* crowded, than
in their ordinary neutral state.   As it may well be admitted that
when two substances are *rubbed* together, their superficial atoms
are displaced in like manner, and as it is known from experience
that friction induces in substances the power of attracting light
bodies, it might reasonably be inferred that the vanes of the
radiometer in motion are capable of similarly attracting.   That
this is actually the case is proved by an experiment which showed
that a pith ball suspended in the neighbourhood of the revolving
vanes is *attracted* (see No. 175 of the ' Proceedings of the Royal
Society,' p. 313, and the theory of this attraction which I have
given in the Number of the Philosophical Magazine for November
1876).

The compression of the superficial atoms on the concave surface
of the cup-shaped radiometer, and the dilatation of those on the
convex surface, would, according to what is said above, be pro-

duced *independently*, and, electrically considered, would act in the same direction, the one answering to *positive*, and the other to *negative*, electricity. Hence, as the amount of compression may be supposed to be equal to that of dilatation, the light incident on both surfaces simultaneously would have *double* the effect of the light incident on either singly. This inference agrees with experimental results.

I have said in the April Number (p. 279) that "the intrinsic molecular forces of the vanes tend continuously to make the atoms return to their neutral positions; but since at the same time the disturbing force is constantly in action, the result of the antagonistic forces will be a permanent abnormal condition of the superficial atoms, so long as the disturbance continues." I propose now to qualify this view by saying that the tendency of the atoms to take the neutral positions is due to *the law of heat-exchanges,* which is always in operation, and which may even be said to include the before supposed action of molecular forces. The thermal influences of the enclosing glass case, and of other surrounding substances, which physicists have taken into consideration in proposed explanations of the action of the radiometer, may be assumed to be embraced by this general law, and on that account may not require to be specifically determined.

<div style="text-align:center">I am, Gentlemen,
Your obedient Servant,
J. Challis.</div>

Cambridge, April 21, 1877.

DIATHERMANEITY OF METALS AND OF PAPER. BY M. AYMONNET.

Conclusions.—My experiments, joined with the observation made by Wiedemann and Frantz[*] on the rapidity with which equilibrium of temperature establishes itself in the bars whose coefficients of conductivity they were seeking, show :—

1st. That the metals and paper are not athermanous, as is generally believed.

2nd. That they are more diathermanous for obscure heat emanating from metallic bodies raised to a temperature below 100° C. than for the luminous heat-radiations, or those near the red.

3rd. That they have feebler absorbing-powers[†] than that of water.

4th. That it is possible to find a mathematical relation between the absorbing-power of a body and its coefficient of conductivity.— *Comptes Rendus de l'Académie des Sciences,* Feb. 5, 1877, tome lxxxiv. p. 259.

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[*] Pogg. *Ann.* vol. lxxxix.

[†] I here call *absorbing-power* the complement of the inverse of the ratio existing between the quantity of heat which normally penetrates into a body and that which issues from it in the same direction.

ON THE REFLECTION OF POLARIZED LIGHT.
BY M. CROULLEBOIS.

Among the fringes the discovery of which we owe to Mr. Airy there is one, called *courbe en semelle* by M. Billet, remarkable for the instability of its form and orientation, and therefore very appropriate to serve as a characteristic. I purpose to show the advantage that may be derived from the study of this curve in order to ascertain :—1, the physical constitution of a mirror (that is to say, its *positive, neutral,* or *negative* nature); 2, the value of the angle of maximum polarization (first constant); 3, the azimuth of restored polarization (second constant).

To obtain this fringe Jamin's apparatus is used, in the ordinary manner, but substituting for the compensator a convergent system formed by a combination of a lens and a perpendicular spar of several millims. thickness. It is, besides, advisable to make use of the homogeneous light of salted alcohol. The mirror giving all the states of ellipticity, the calculation of the isochromatic curves must be effected in a very general case.

Suppose two axes traced in a plane perpendicular to the reflected ray :—the one horizontal, OX, lying in the first azimuth (the plane of incidence); the other vertical, OY. Let $OP = 1$ be the initial vibration, having its direction in the upper right-hand quadrant; and let α, β, γ be the angles formed respectively with OX by the vibration OP, by the principal section of the plate, and by that of the polariscope. The definitive extraordinary image is formed of four components, of which the amplitudes and phases are :—

$$A = \quad \cos \sigma \cos \beta \cos (\gamma - \beta), \quad \phi + E ;$$
$$B = \quad \sin \sigma \sin \beta \cos (\gamma - \beta), \quad \phi' + E ;$$
$$C = \quad \sin \sigma \cos \beta \sin (\gamma - \beta), \quad \phi' + O ;$$
$$D = - \cos \sigma \sin \beta \sin (\gamma - \beta), \quad \phi + O .$$

ϕ and ϕ' designate the phases introduced by reflection, O and E those of the two rays in the spar. In addition, we have put

$$h \cos \alpha = \cos \sigma, \quad k \sin \alpha = \sin \sigma.$$

To calculate the intensity I of the image, we further put

$$\phi - \phi' = \delta, \quad O - E = \delta',$$

and we find the following value—

$$I = (\Sigma A \cos \xi)^2 + (\Sigma A \sin \xi)^2.$$

The isochromatic curves are given by the equation

$$\frac{dI}{d\delta'} = 0,$$

which, rendered explicit, gives

$$\tan \delta' = \frac{- \sin 2\sigma \sin \delta}{\cos 2\sigma \sin 2\beta - \sin 2\sigma \cos 2\beta \cos \delta} = \tan 2\pi\rho^2,$$

ρ being proportional to the distance from the centre of the field to

the point where the luminous ray strikes the retina. Such is the polar equation of the sole-curved fringe (*frange en semelle*).

This fringe has two axes of symmetry of unequal length. The shorter axis is given, in orientation and in length, by the two formulæ :—

$$\tan \beta = - \frac{1}{\tan 2\sigma \cos \delta},$$

$$\tan 2\pi \rho^2 = \frac{\sin 2\sigma \sin \delta}{\sqrt{1 - \sin^2 2\sigma \sin^2 \delta}}.$$

Hence the following consequences :—

1. If $\delta > 0$ (that is, if the mirror is *positive*), the minor axis is oriented in the upper right-hand quadrant.

2. If $\delta < 0$ (that is, if the mirror is *negative*), the same axis is oriented in the upper left-hand quadrant.

3. If $\delta = 0$ (that is, if the mirror is *neutral*), the minor axis vanishes.

4. If $\delta = \pm \frac{\pi}{2}$ (that is to say, under the principal incidence), the minor axis remains oriented at 45°, whatever may be the orientation of the initial vibration.

5. Under this incidence, when the polarizer is rotated, Airy's fringes become transformed into Dove's rings. Let α be the azimuth obtained; it is that of the restored polarization, connected with the factors of alteration of the amplitudes by the formula $\tan \alpha = \frac{h}{k}$.

This method is convenient for the study of reflection in general and also of the laws of Cauchy and Jamin ; moreover it is delicate. Indeed, if you employ ordinary glass as the mirror, and observe under an incidence near the polarization-angle, you will recognize that the extremities of the minor axis have an appreciable deviation ; and yet the elliptical polarization is so little evident that it escaped Fresnel.

An interesting experiment consists in arranging in succession three mirrors—of *steel*, of *alum*, and of *fluorine*. The first and the third, being one positive, the other negative, present two contrasting positions of the fringe *en semelle* ; the second, being neutral, offers the transitory phenomenon of conserved rectilineal polarization.—*Comptes Rendus de l'Académie des Sciences*, March 26, 1877, tome lxxxiv. pp. 604–606.

NOTE ON MOLECULAR VOLUMES.   BY F. W. CLARKE, S.B.,
PROFESSOR OF PHYSICS AND CHEMISTRY.

Several years ago, in a series of papers upon atomic or molecular volumes, I pointed out some curious multiple relations connecting both elements and compounds*. For example, I found that a simple relation of this kind connected the alkaline metals with each other;

* Silliman's American Journal, March and May, 1869 ; September, 1870.

and, later, that the haloid salts of some of these metals had molecular volumes multiples of that of hydrogen. For this latter relation, however, my materials were meagre. I had then the specific-gravity determinations for LiCl, NaCl, KCl, NaBr, KBr, NaI, and KI, or seven compounds in all. To these I added, though unsatisfactorily, the corresponding salts of silver, making a list of ten bodies closely related, and giving volumes multiples of 5·5, the value assigned by Kopp to hydrogen in its liquid compounds at their boiling-points. This relation I am now able to extend, partly by new density-observations of my own, to include at least twelve compounds not in my earlier list.

My own determinations, in addition to those I have already given for the alkaline fluorides, are as follows:—Rubidium chloride, 2·209, 19°; rubidium bromide, 2·780, 17°·5; rubidium iodide, 3·023, 22°; lithium bromide, 3·102, 17°; lithium iodide, 3·485, 23°.

Now let us tabulate the material. The first column contains the symbol of the substance, the second its density with authority given, the third its molecular volume as found, the fourth its volume calculated, the fifth a theoretical density deduced from this volume. The calculated volumes are of course the exact multiples of Kopp's hydrogen value, and will be seen at once to agree closely with the results of experiment. The real variation between fact and theory, however, will be best seen upon comparing the two columns of densities. The differences here are always less than 0·1.

I.	II.	III.	IV.	V.
LiF	2·295, Clarke.	11·33	11·00	2·363
LiCl	1·998, Kremers.	21·27	22·00	1·932
LiBr	3·102, Clarke.	28·05	27·50	3·164
LiI	3·485, ,,	38·45	38·50	3·481
NaF	2·558, ,,	16·41	16·50	2·545
NaCl	2·145, Bruignet.	27·27	27·50	2·127
NaBr	3·079, Kremers.	33·45	33·00	3·121
NaI	3·450, Filhol.	43·48	44·00	3·409
KF	2·096, Clarke.	28·20	27·50	2·113
KCl	1·945, Kopp.	38·35	38·50	1·937
KBr	2·672, Playfair, Joule.	44·57	44·00	2·707
KI	3·056, Filhol.	54·35	55·00	3·020
RbF	3·202, Clarke.	32·64	33·00	3·167
RbCl	2·209, ,,	54·78	55·00	2·200
RbBr	2·780, ,,	59·53	60·50	2·735
RbI	3·023, ,,	70·29	71·50	2·972

Here, now, we have sixteen compounds of a single type, every one of which agrees with the rule. In each case the molecular volume comes out a multiple of 5·5, or very nearly. Only one substance in the list seems to be in any way abnormal, namely, rubidium fluoride, with its volume of 33. The other fluorides in this group have volumes less by 11 than those of the corresponding chlorides; but in this case the difference is 22. A curious progressive relation is also worth noting. If we compare the five chlorides given in the Table we shall see that, upon arranging them in the order of their molecular weights, the differences between successive members of the series increase as we ascend. Thus LiCl and NaCl differ by 5·5,

NaCl and KCl by 11, KCl and RbCl by 16·5. This regular difference-increase is certainly suggestive of some law yet to be clearly made out.

A similar relation to Kopp's hydrogen-volume is also afforded by the two other compounds. Sodium hydride (Na_2H), discovered by Troost and Hautefeuille*, with a density of 0·959, has a molecular volume of 49·1, or very nearly $5·5 \times 9$. Still more interesting is iodine monochloride, so carefully studied by Hannay†. At 0° the solid substance has a specific gravity of 3·263, and a molecular volume of 49·8, thus varying only 0·3 from a multiple of 5·5. At 101° the chloride boils, and at 98° its density is 2·958, having a volume of 54·9. Probably an absolutely correct determination at its boiling-point would give a value of 55. So we may say that iodine monochloride, both as a solid at 0°, and as a liquid at its boiling-point, has molecular volumes multiples of that of hydrogen.

As for the haloid salts of silver, they cannot with certainty be included among the substances connected by this multiple relation. The fluoride agrees fairly, however, having a density of 5·852, Gore, and a molecular volume of 21·7 instead of 22. The chloride and iodide may be forced to agree by selecting out the density-determinations of certain investigators, and rejecting other decidedly discordant data. The bromide does not agree at all. A determination of my own for precipitated AgBr gives a density of 6·215, 17°, and a corresponding volume of 30·25. Other determinations are even more discordant than this. Silver salts generally have molecular volumes equal or nearly equal to the corresponding sodium compound, that of sodium bromide being 33·0. Silver fluoride, it will be seen, diverges also from the sodium salts. For thallium our data are insufficient. Its monochloride has a molecular volume approximating to a multiple of 5·5, but not closely enough to be satisfactory ; the sesquichloride does not even approximate. At some future time I hope to be able to revise and extend our specific-gravity determinations for this class of thallium salts.

Now to sum up. Including the silver and thallium salts we know the densities of twenty-five substances containing only univalent elements. Of these, twenty have molecular volumes multiples of that of hydrogen, three are doubtful, two apparently disagree. We may therefore safely assert the following general law, subject to possible exceptions :—*Every compound containing only elements of the hydrogen group has a molecular volume an even multiple of that of hydrogen.* This is probably but a hint of some more general regularity connecting other elements and other groups.

Postscript.—Since the above pages were written, there has been published by Johnson a density-determination for potassium tri-iodide, KI_3 (Chem. News, vol. xxxiv. p. 256). This determination, 3·498, corresponds to a molecular volume of 120·1. 121 is an exact multiple of 5·5, and gives a theoretical density of 3·472. The multiple relation now holds good in twenty cases out of twenty-five.—Silliman's *American Journal*, April 1877.

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* *Comptes Rendus,* vol. lxxviii. 970.   † Journ. Chem. Soc. II. xi. 818.

LV. *On some Effects of Dissociation on the Physical Proper-*
ties of Gases. By W. M. HICKS, *M.A., Fellow of St. John's*
College, Cambridge[*].

THE following pages contain an attempt to discover what
effect the dissociation of an elementary or compound gas
has upon its physical constants. That dissociation must exist
to some extent at all temperatures is exceedingly probable;
and if so, it must necessarily affect the physical properties of
the gas; and especially we might suppose that it would pro-
duce variation from Charles's law, and perhaps explain the
difficulty hitherto experienced in accounting for the ratio
1·408 of the specific heats of a permanent gas. It was under
this belief, and also with some hope of throwing a little light
on the chemical changes which take place in compound gases,
and on mixing different gases, that I undertook the mathema-
tical investigation of it.

I.

1. Before we can apply mathematical reasoning to the con-
sideration of dissociation, it will be necessary to have some
hypothesis on the manner in which it takes place. Our hypo-
thesis should be one which, while being as simple as possible,
is likely to contain the essentials of what really takes place,
even though it may not be correct in all its details. Dissocia-
tion of a compound gas is that state in which the compound
molecules of a gas are split up into their component parts and
exist together uncombined. I shall suppose the same also to

[*] Communicated by the Author.

take place in an elementary gas, the molecule being composed of two atoms. How the atoms are bound together we do not know ; but, from what we can gather, there seems to be some attractive force between them which at very close quarters changes into a repulsive one. Equilibrium is sustained by the attraction between the two atoms and their motion about one another. If, then, the two atoms of a molecule become separated, there seem only two ways of accounting for it. Either their relative motion becomes so large as to overcome the force of attraction ; or some external force must act upon them, which can be nothing else than a reaction between them and some other molecule. The latter is the hypothesis I have adopted in the following investigation.

2. I consider the atom to be smooth, spherical, and perfectly elastic, and, in order to bring the dissociation under mathematical treatment, suppose

(1) That when a molecule experiences a blow greater than a certain blow c, it breaks up into its component atoms,

(2) That when two atoms impinge with a blow less than c, they combine to form a molecule.

Now it is exceedingly improbable that any of these suppositions is absolutely true ; but yet I venture to think, since the mathematical form would be similar, that the state of such a gas would differ only slightly from that of real gases. As was said before, the reaction between two molecules is probably a varying one, and it is unlikely that they ever come into real contact ; still the mean effect will be similar to the case under our hypothesis. We must look upon c as a mean blow for different directions of incidence, or as some quantity which in the real state determines whether the molecules will break up or not. For instance, if the force between two atoms were inversely as the square of the distance, c would determine whether the resulting orbit of two atoms coming together would be an ellipse, or a parabola or hyperbola. So also the radii of action must be taken as average quantities. Further, it is not likely that they are absolutely independent of the temperature ; for it is conceivable that as the internal energy increases they will fly further apart, and thus become more liable to blows from the other molecules. Neither are we perhaps warranted in assuming that c is constant ; since c constant involves the invariability of the distance of the two atoms ; for if the distance increased, the force between would diminish, and therefore c also. Nevertheless, although numerical results would be affected by this cause, the general laws would be of the same form in the two cases ; and in either case our experimental knowledge is neither wide

enough, nor exact enough, to enable us to deduce exact numerical results.

3. In the present paper I first consider the problem, To find the number of blows greater than c, between two sets of moving particles, per unit of time, in unit of volume. This is then applied to find the dissociation at any temperature, and a quadratic obtained, the positive root of which gives the ratio of molecules to free atoms in the gas ; and it is shown that the resulting state of the gas is a stable one. This ratio is used to investigate the relation between the temperature and pressure ; and it is shown that Charles's law is not rigorously exact, though within ordinary ranges of temperature it is very nearly so ; reasons are also given why for the permanent gases c should be large, compared with the mean blow at ordinary temperatures. Next the specific heats are considered ; and it is proved that for a diatomic molecule, in which the atoms are smooth and spherical, or the energy of rotation of the atoms is unaffected by external causes, the ratio of the specific heats is about 1·4 ; it is also shown that the specific heats are almost rigorously constant at ordinary temperatures. This concludes the present paper ; in what is to follow I intend to discuss the properties of a compound gas of the type HCl.

4. When we pass to the consideration of a compound gas, even of the simplest form, additional difficulties are introduced, as equations of a high order appear whose algebraical solution is impossible, and from which therefore it will be extremely difficult to deduce general laws. All we seem able to do is to take one or two particular cases, and learn what we can about them. For instance, in the case of a gas of the type HCl, *i.e.* in which two monatomic gases combine to form a gas whose molecule is diatomic, we get three equations of the second degree between three unknowns. In general, when two gases A and B are mixed together, there will always be a certain proportion of a new gas C whose molecule is composed of atoms of A and B, and the proportions of free atoms and molecules of A and B will be altered. When the excess of the number of combinations of free atoms of A with B over the number of molecules of C destroyed in any time is greater than the number of combinations of free atoms of A to form A and of free atoms of B to form B, then the proportion of A and B decreases, whilst that of C increases, and we get a chemical change. This will explain why often the mere presence of another gas D will produce a change of A and B into C ; for the action of D on A and B may cause more of their molecules to be broken up than of C, while at the same

time not many more of its own are ; therefore, since the combinations of A and B atoms into C molecules are destroyed more slowly than the combinations of A and B atoms into A and B molecules, the gases A and B after a time change into C.

5. In a similar manner we may explain the result obtained by Andrews, that if nitrogen be mixed with carbon dioxide, the mixture may be subjected to very high pressure without the carbon dioxide being condensed. When a gas condenses, we must suppose that molecules combine with molecules to form larger aggregations, and that this continues until the whole gas is condensed into a liquid. When the temperature is below a certain degree, the change from the gaseous to the liquid state takes place around nuclei, in which the molecules get entangled, and which the energy of agitation is not great enough to destroy ; in this case a mist is formed, and we see the gas condense. But when the temperature is higher than a certain degree (the critical temperature), the energy of agitation is so great that an aggregation greater than the average gets broken up as soon as formed, and the gas passes into the liquid form by having its particles uniformly pressed more closely together ; no separate nuclei are formed to diffuse the light passing through, and we can see no change take place. The nitrogen molecules and atoms act in the same way to break up the carbon dioxide aggregations as soon as formed, and hence prevent it from condensing.

6. The fact that two states of a gas are possible under the same conditions, as for instance $2 N + O$ and $N_2 O$, is easily explicable, as it is quite clear that in general there will be more than one way in which the losses of the different molecules may be equal to their gains. In other words, the equations of high order, which we obtain, have several positive roots, which give a stable state of the gas : as the temperature rises, one such state may become unstable ; in this case we shall get a sudden change into one of the other stable states, with a transformation of energy, answering in experience to an explosion with evolution of heat.

II.

7. Let N be the number of molecules of mass m_1 in unit of volume, and N′ the number of molecules of mass m_2. Also let α, β be the velocities of mean square in the two systems respectively, and s the mean effective distance between molecules of the first kind and of the second.

Consider a molecule of the first kind moving with velocity v; the number of such whose velocities lie between v and $v + \delta v$

is $\dfrac{4\mathrm{N}}{\sqrt{\pi}\alpha^3}v^2 e^{-\frac{v^2}{\alpha^2}}\delta v^*$; and let us find the number of collisions in unit of time between one of these (m_1) and the second system of molecules. The number of molecules of the second kind in unit of volume whose velocities lie between u and $u+\delta u$ is $\dfrac{4\mathrm{N}'}{\sqrt{\pi}\beta^3}u^2 e^{-\frac{u^2}{\beta^2}}\delta u$. The proportion of those moving at angles between θ and $\theta+\delta\theta$ with the direction of motion of m_1 is $\frac{1}{2}\sin\theta\delta\theta$.

The relative velocity of m_1 with these

$$=\sqrt{u^2+v^2+2uv\cos\theta}=r,\text{ say };$$

and the number of collisions in unit of time is

$$\pi s^2 r \cdot \frac{4\mathrm{N}'}{\sqrt{\pi}\beta^3}u^2 e^{-\frac{u^2}{\beta^2}} \cdot \tfrac{1}{2}\sin\theta\,\delta u\,\delta\theta = \overline{\mathrm{N}}\,r\sin\theta\,d\theta,\text{ say.}$$

8. We have now to find the proportion of those that strike with a blow greater than a given quantity (c, say). Consider a molecule of the second system striking m_1 at an arcual distance ϕ from the point of direct impact. The relative velocity of the surfaces before impact is $r\cos\phi$, and as the molecules are supposed perfectly elastic, the blow will be $\dfrac{2m_1 m_2}{m_1+m_2}r\cos\phi$; and this must be $>c$. If, therefore, ϕ_1 be such an angle that $2m_1 m_2\,r\cos\phi_1=(m_1+m_2)c$, all those molecules that fall within a small circle whose radius is $s\sin\phi_1$ will strike with a blow $>c$. The proportion required of the whole number of impacts is therefore

$$\frac{\pi s^2\sin^2\phi_1}{\pi s^2}=\sin^2\phi_1=1-\left(\frac{(m_1+m_2)c}{2m_1 m_2 r}\right)^2=1-\left(\frac{c'}{r}\right)^2,\text{ say };$$

and the whole number of collisions required

$$=\overline{\mathrm{N}}r\left\{1-\left(\frac{c'}{r}\right)^2\right\}\sin\theta\,\delta\theta.$$

9. The number of molecules of the second kind with velocities between u and $u+\delta u$, and making an angle between θ and $\theta+\delta\theta$ with direction of m_1, is

$$\frac{2\mathrm{N}'}{\sqrt{\pi}\beta^3}\sin\theta\delta\theta u^2 e^{-\frac{u^2}{\beta^2}}\delta u.$$

Therefore the number of those with velocity u which have a

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* See Maxwell, Phil. Mag. January 1860.

velocity r relative to m_1

$$= \frac{2N'}{\sqrt{\pi}\beta^3} \sin\theta_1 \, \delta\theta u^2 e^{-\frac{u^2}{\beta^2}} \delta u,$$

where

$$u^2 + v^2 + 2uv \cos\theta_1 = r^2.$$

For the same value of u when r becomes $r + \delta r$, θ_1 becomes $\theta_1 + \delta\theta_1$, where

$$\sin\theta_1 \delta\theta_1 = -\frac{r}{uv}\delta r,$$

and the above number becomes

$$-\frac{2N'u}{\sqrt{\pi}\beta^3 v} e^{-\frac{u^2}{\beta^2}} \delta u \, r \delta r.$$

To find the number of molecules impinging on m_1 with relative velocity between r and $r + \delta r$, this must be multiplied by $\pi s^2 r$ and integrated with respect to u over all possible values of u, i. e. for such that $\dfrac{r^2 - u^2 - v^2}{2uv}$ lies between ± 1, or

$$u + v > r,$$
$$u \sim v < r.$$

The number required is therefore

$$= \frac{2\sqrt{\pi}N's^2 r^2}{\beta^3 v}\delta r \int_{r-v}^{r+v} u e^{-\frac{u^2}{\beta^2}} \, du$$

$$= \frac{\sqrt{\pi}N's^2 r^2}{\beta v}\delta r \{ e^{-\frac{(r-v)^2}{\beta^2}} - e^{-\frac{(r+v)^2}{\beta^2}} \} dv.$$

The proportion of those impinging with a blow $> c$ is $1 - \dfrac{c'^2}{r^2}$.

Hence the whole number of impacts of the second system on m_1 with a blow $> c$ is

$$\frac{\sqrt{\pi}N's^2}{\beta v} \int_{c'}^{\infty} (r^2 - c'^2)\{ e^{-\frac{(r-v)^2}{\beta^2}} - e^{-\frac{(r+v)^2}{\beta^2}} \} \, dr \, ;$$

and the whole number of blows $> c$ between molecules of the first and second kinds per unit of time is

$$\frac{4NN's^2}{\alpha^3 \beta} \int_0^{\infty} \int_{c'}^{\infty} (r^2 - c'^2)\{ e^{-\frac{(r-v)^2}{\beta^2}} - e^{-\frac{(r+v)^2}{\beta}} \} v e^{-\frac{v^2}{\alpha^2}} \, dv \, dr.$$

This may be put in the form

$$\frac{4NN's^2}{\alpha^3 \beta} \int_{c'}^{\infty} (r^2 - c'^2)e^{-\frac{r^2}{\alpha^2+\beta^2}} \, dr \int_0^{\infty} \{ e^{-\frac{(v-h)^2}{\gamma^2}} - e^{-\frac{(v+h)^2}{\gamma^2}} \} v \, dv,$$

where

$$h = \frac{\alpha^2 r}{\alpha^2 + \beta^2} \text{ and } \gamma^2 = \frac{\alpha^2 \beta^2}{\alpha^2 + \beta^2}.$$

The second integral

$$= \int_{-\infty}^{+\infty} (v+h) e^{-\frac{v^2}{\gamma^2}} dv = h\sqrt{\pi}\gamma = \frac{\sqrt{\pi}\alpha^3 \beta r}{(\alpha^2 + \beta^2)^{\frac{3}{2}}}.$$

Hence the whole integral

$$= \frac{4\sqrt{\pi} \mathrm{NN}' s^2}{(\alpha^2 + \beta^2)^{\frac{3}{2}}} \int_{c'}^{\infty} (r^2 - c'^2) r e^{-\frac{r^2}{\alpha^2 + \beta^2}} dr$$

$$= 2\sqrt{\pi} \mathrm{NN}' s^2 \sqrt{\alpha^2 + \beta^2} \, e^{-\frac{c'^2}{\alpha^2 + \beta^2}}.$$

Let $\frac{\theta}{2}$ be the common mean kinetic energy of molecules in each system. Then

$$m_1 \alpha^2 = m_2 \beta^2 = \theta;$$

$$\therefore \alpha^2 + \beta^2 = \frac{m_1 + m_2}{m_1 m_2} \theta;$$

also

$$c' = \frac{m_1 + m_2}{2 m_1 m_2} c.$$

Hence the number of blows $> c$ in a unit of volume per unit of time between molecules of the first and second kinds respectively is

$$2\sqrt{\pi} \mathrm{NN}' s^2 \sqrt{\frac{m_1 + m_2}{m_1 m_2}} \sqrt{\theta} e^{-\frac{m_1 + m_2}{4 m_1 m_2} \cdot \frac{c^2}{\theta}}.$$

The law of variation of the number for the same pair of gases is of the form

$$\mathrm{A}\rho\rho' \sqrt{t} \, e^{-\frac{t_0}{t}},$$

where ρ, ρ' are the densities of the two gases, t is the absolute temperature, and A and t_0 are constants. It is probable that c or t_0 will vary with the amount of internal energy of the molecules, and will diminish as the temperature increases.

Putting $c = 0$, we find the whole number of collisions to be

$$2\sqrt{\pi} \mathrm{NN}' s^2 \sqrt{\frac{m_1 + m_2}{m_1 m_2}} \sqrt{\theta},$$

as has been shown by Maxwell[*]. Hence the number of colli-

[*] Phil. Mag. January 1860

sions which impinge with a blow $< c$

$$= 2\sqrt{\pi}\mathrm{NN}'s^2\sqrt{\frac{m_1+m_2}{m_1 m_2}}\left(1 - e^{-\frac{m_1+m_2}{4m_1 m_2}\cdot\frac{c^2}{\theta}}\right)\sqrt{\theta}.$$

10. The foregoing expression enables us to find the mean blow at any temperature, thus. The number of blows $> c$ is given by an expression of the form $\lambda e^{-\mu c^2}$; hence the number of blows between c and $c + \delta c$

$$= 2\lambda\mu c e^{-\mu c^2}\delta c ;$$

hence the mean blow

$$= \frac{2\lambda\mu\displaystyle\int_0^\infty c^2 e^{-\mu c^2}\,dc}{\text{No. of blows} > 0}$$

$$= \frac{2}{\sqrt{\mu}}\int_0^\infty x^2 e^{-x^2}\,dx = \frac{1}{\sqrt{\mu}}\int_0^\infty e^{-x^2}\,dx = \tfrac{1}{2}\sqrt{\frac{\pi}{\mu}} ;$$

hence, calling the mean blow c',

$$c'^2 = \frac{\pi}{4\mu} = \frac{\pi}{4}\cdot\frac{4m_1 m_2}{m_1+m_2}\,\theta = \pi\cdot\frac{m_1 m_2}{m_1+m_3}\cdot\theta,$$

or c' is proportional to the square root of the temperature. We may therefore express the number of blows $> c$ in the form

$$2\mathrm{NN}'s^2\left(\frac{m_1+m_2}{m_1 m_2}\right)c'e^{-\frac{c^2}{c'^2}}.$$

III. *Case of an Elementary Gas.*

11. We will now employ the foregoing formula to investigate the effect of dissociation on an elementary gas whose molecule is diatomic. We shall suppose the dissociation to occur through a molecule receiving a blow $> c$; also that if two atoms come within a mean distance s_2 so that they would impinge with a blow $< c$, they will combine into a molecule.

Let then $x =$ number of molecules in unit of volume,

$2y =$ number of free atoms in unit of volume,

so that $2(x+y) =$ whole number of atoms present $= \mathrm{N}$, say.

Further, let $m =$ mass of an atom ;

s, s_1, s_2 be the mean effective distances between two molecules, a molecule and an atom, and two atoms.

Then the number of collisions of molecules with a blow $> c$

$$= 2x^2\sqrt{\pi}\,s^2\sqrt{\frac{\theta}{m}}\,e^{-\frac{c^2}{4m\theta}}.$$

$=$ half the number of molecules destroyed   The number of

collisions of a molecule and atom with a blow $> c$

$$= 4xy \sqrt{\pi} \, s_1^2 \sqrt{\frac{3\theta}{2m}} \, e^{-\frac{3c^2}{8m\theta}}$$

$=$ the number of molecules destroyed. The number of collisions of two atoms with a blow $< c$

$$= 8y^2 \sqrt{\pi} \, s_2^2 \sqrt{\frac{2\theta}{m}} \, (1 - e^{-\frac{c^2}{2m\theta}}).$$

Hence the whole gain of molecules in a unit of time is

$$\frac{dx}{dt} = \{2y^2 s_2^2 \sqrt{2}(1 - e^{-\frac{c^2}{2m\theta}}) - xy s_1^2 \sqrt{\tfrac{3}{2}} \, e^{-\frac{3c^2}{8m\theta}} - x^2 s^2 e^{-\frac{c^2}{4m\theta}}\} 4\sqrt{\frac{\pi\theta}{m}}$$

When the temperature is constant $\frac{dx}{dt}$ must $= 0$, which condition, with $2(x+y) = N$, gives us equations to find x. Putting

$$\frac{x}{y} = \xi, \quad \frac{c^2}{4m} = \theta_0,$$

the condition becomes

$$\xi^2 + \xi \cdot \sqrt{\tfrac{3}{2}} \left(\frac{s_1}{s}\right)^2 e^{-\frac{\theta_0}{2\theta}} - 2\sqrt{2} \cdot \left(\frac{s_2}{s}\right)^2 (e^{\frac{\theta_0}{\theta}} - e^{-\frac{\theta_0}{\theta}}) = 0 ;$$

say

$$\xi^2 + 2a\xi - b = 0,$$

whence

$$\xi = \pm \sqrt{a^2 + b} - a.$$

12. The negative root has no meaning, whence we must take the positive root. But before we can assert that this value of ξ gives the actual proportion, we must see whether the state of the gas would be stable. The condition for this is clearly that, if x be increased, $\frac{dx}{dt}$ must be negative, and *vice versâ.* Writing

$$\frac{dx}{dt} = f(x, y), \quad x + y = \frac{N}{2},$$

we have when x is increased to $x + h$, and therefore y to $y - h$,

$$\frac{dx}{dt} = f(xy) + h\left(\frac{df}{dx} - \frac{df}{dy}\right) = h\left(\frac{df}{dx} - \frac{df}{dy}\right);$$

we must therefore have $\frac{df}{dx} - \frac{df}{dy} =$ negative quantity. In this particular case

$$f(xy) = \lambda(by^2 - 2axy - x^2),$$

where λ, a, b are positive. The condition for stability, there-

fore, is that $-x-ay-by+ax$ must be negative, or

$$\xi+\overline{a+b}>a\xi,$$

or, substituting for ξ,

$$1>a-\sqrt{a^2+b},$$

which is clearly the case, and the value of ξ therefore gives a stable state.

The condition may be stated in a different form as follows. When x increases through the root, $f(xy)$ must change from positive to negative. From this again we see that the positive root of $by^2-2axy-x^2=0$ gives a stable state. For when $x=0$ the expression is positive, and when $x=\infty$ it is negative, whence, as there is only one positive root, $f(xy)$ must change from positive to negative as x increases through it.

13. The expression above found for ξ gives twice the ratio of molecules to free atoms in the gas at any given temperature θ. The proportion of molecules to the whole number of moving particles in the gas is therefore $=\dfrac{\xi}{2+\xi}=\eta$ suppose whilst the whole number of moving particles $=2y+x=\frac{1}{2}\dfrac{2+\xi}{1+\xi}$ N.

In order to obtain some idea of the law of variation of the proportions of molecules and free atoms with the temperature, I have traced the curve in fig. 1. The abscissæ denote the tem-

Fig. 1.
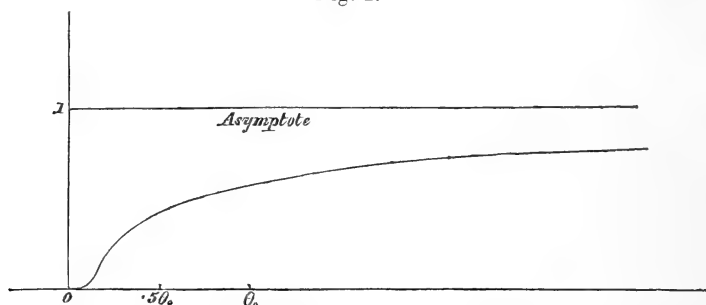
perature measured in terms of θ_0, whilst the ordinates give the proportion of free atoms to moving particles. The particular curve represented belongs to the case where $s_1=\dfrac{s+s_2}{2}$ (which seems very likely the case) and $s=\frac{3}{2}s_2$ (or the radius of action of a molecule equal to the sum of the radii of action of the atoms of which it is composed); but the general *form* of the curve does not depend on the values of s, s_1, s_2, and only varies very slightly with their variations. It is noticeable that there

are two periods when the variation of the proportions is small, viz. when the temperature is small compared with θ_0 (less than $\frac{1}{10}\theta_0$) and when it is greater than θ_0. The curve, of course, has an asymptote at a distance 1 from the origin; if we regard this as the line of abscissæ, the ordinates measured from it give the values of η.

14. In considering the pressure of the gas we may treat it as a mixture of two. We shall therefore have

$$p = \tfrac{2}{3} . 2mxv_1^2 + \tfrac{2}{3}m . 2yv_2^2,$$

where v_1, v_2 are the velocities of mean square in the two cases; and therefore $2mv_1^2 = mv_2^2 = \lambda\theta$, where θ is the absolute temperature and λ some constant, whence

$$p = \tfrac{2}{3}(x+2y)\lambda\theta = \frac{N}{3}\frac{(2+\xi)}{1+\xi}\lambda\theta = \frac{N}{3}\left(1 + \frac{1}{1+\xi}\right)\lambda\theta$$
$$= \kappa(1+\zeta)\rho\theta, \text{ say.}$$

If there were no dissociation, we should have, calling P the pressure in this case,
$$\text{P} = \kappa\rho\theta ;$$
hence
$$p = (1+\zeta)\text{P}.$$

Fig. 2 shows the relations of p and P in a gas where $s = \tfrac{3}{2}s_2$;

Fig. 2.
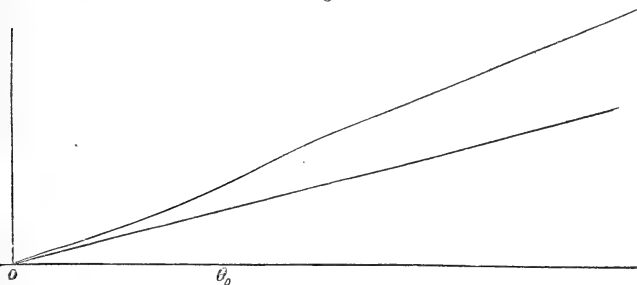
the abscissæ represent the temperature, and the ordinates the corresponding pressures in the two cases. The bend in the curve between $\cdot 5\theta_0$ and $1\cdot 5\theta_0$ is noticeable.

15. According to the ordinary theory $\text{P} = \kappa\rho t$, where t is the temperature measured from a certain zero-point, which is very slightly different for the different permanent gases. According to the above theory $p = \kappa\rho(1+\zeta)\theta$, where θ is the mean kinetic energy of translation of the particles composing the gas. Now, in the formula $\text{P} = \kappa\rho t$, the temperatures are

measured by comparison with the temperature of mercury or some other substance ; and all we can deduce is that when the temperature increases a certain degree, the pressures of all the permanent gases increase in very nearly the same ratio with one another ; but we have no proof that what we consider equal increments of temperature corresponds to exactly equal increments of mean kinetic energy.   This is generally acknowledged and the temperatures measured by the "air-thermometer" are supposed to be correct, whilst the variations from the law $P = \kappa \rho t$ (when t is measured by the mercury-thermometer) are set down to the account of the unequal expansibility of mercury at different temperatures.   But even on this supposition all that experiment tells us is that, in the formula $P = \kappa \rho t$, κ is the same for all permanent gases, and t is measured from nearly the same zero-point.   For instance, suppose that ζ is the same for all gases at the same temperature ; then, $(1 + \zeta)\theta$ being the same at the same temperature for all gases, we should find in our experiments that κ is the same and t is measured from the same zero.   But if we could by some means measure θ correctly and then apply our corrections to the temperatures as given by the mercury-thermometer, we should find that the coefficient of expansion of mercury increases still more with the real temperature than is apparent from our comparisons of air and mercury, supposing, as is done, that equal increments of the pressure of air correspond to equal increments of temperature, and not, as on the theory of dissociation, to equal increments of the quantity $(1 + \zeta)\theta$.

Since it is highly probable that the relations between s, s_1, s_2 are the same for all gases, the only condition that $(1 + \zeta)\theta$ may be the same is that θ_0 or $\dfrac{c^2}{m}$ may be the same, which would mean that, if two molecules are to break up, the relative velocity of the two impinging directly must be such—not that the minimum *blow* is the same in all gases, but that the *vis viva* must be the same, or that the force of cohesion is proportional to the square root of the mass.   As I see no likely hypothesis on which this may be the case, I think it is not probable that $(1 + \zeta)\theta$ is rigorously independent of the kind of gas.   But that the theory may agree with experiment, it is not necessary that $(1 + \zeta)\theta$ should be rigorously the same for all gases ; in fact, experiment tells us it is not so, although the variation is very small.

16. If for all the permanent gases θ_0 be small compared with our ordinary temperatures, then our ordinary temperatures are some multiples of θ_0, and the range within which

experiments have been made is also some multiple of θ_0. Moreover a slight difference in the values of θ_0 for the different gases would give a greater difference in the law of pressure as the temperature increases. If, on the other hand, θ_0 be great, so that ordinary temperatures are $< \frac{1}{10}\theta_0$, then as we have seen before, the variation and dissociation is small, and the more so as the maximum range of the Centigrade temperatures would all be small fractional parts of θ_0. Here also variations of θ_0 would not produce much difference between different gases at ordinary temperatures. We should be led therefore to suppose that θ_0 is in general large. This is also supported by the fact that it is highly probable that at ordinary temperatures the dissociation of the permanent gases is small. For the sake of illustrating this and also to obtain some idea of the magnitude of the variations involved, we will consider more fully the case of two gases, in one of which $\theta_0 = 100°$, and in the other $= 5000°$, the degrees being Centigrade and measured from absolute zero, *i. e.* $-273°$ C.

17. In Tables I. and II. below, column 1 gives the temperature; column 2 the dissociation at the given temperature, the dissociation being measured by the proportion of free atoms to moving particles in the gas; column 3 gives the value of $1 + \zeta$ in the formula $p = \kappa\rho(1+\zeta)\theta$, and which we may call the expansibility; whilst column 4 gives the ratio $\dfrac{\frac{p}{\theta}}{\frac{p_0}{\theta_0}}$ (p_0, θ_0 being the pressure and temperature at 27° C.).

<div align="center">Table I. ($\theta_0 = -173°$ C.).</div>

θ.	$\dfrac{2y}{x+2y}$.	$1+\zeta$.	$\dfrac{\frac{p}{\theta}}{\frac{p_0}{\theta_0}}$.
-273	0	1	
-173	·610	1·439	0·873
-73	·728	1·571	0·953
27	·785	1·647½	1
127	·820	1·695	1·029
227	·845	1·732	1·051
527	·858	1·752	1·063
∞	1	2	1·21

TABLE II. ($\theta_0 = 5000° = 4727°$ C.).

θ.	$\dfrac{2y}{x+2y}$.	$1+\zeta$.	$\dfrac{\frac{p}{\theta}}{\frac{p_0}{\theta_0}}$.
-273	0	1	
-73	·0000	1·0000	
27	·0017	1·0008	1
127	·0096	1·0048	1·0048
227	·0269	1·0136	1·0136
327	·057	1·0268	1·0260
527	·116	1·0617	1·0610
727	·182	1·1003	1·0994
1227	·313	1·1850	1·1841
1227	·459	1·2970	1·2961
∞	1	2	1·9984

18. From Table II. we see that at ordinary temperatures the dissociation is very small when $\theta_0 = 5000°$, even up as high as 700° C.; whilst when $\theta_0 = 100°$ or $-173°$ C., the dissociation is always large, even down to $-200°$ C. Also the expansibility in the first case between temperatures 0° C. and 100° C. varies as much as in the second case between 0° C. and 300° C., though when we get to high temperatures the expansibility changes more slowly in the first case than in the second. In the second case the expansibility is almost constant between 0° C. and 100° C., and even up to 300° changes only slightly. Again, a difference of 10 per cent. in the value of θ_0 in different gases will produce a difference in their expansibilities at ordinary temperatures of ·004 when θ_0 is small and about $-173°$ C., and of ·00065 when θ_0 is large and about 4727° C. In other words, if θ_0 be $-173°$ C. in one gas and the value of θ_0 in the other gas vary to the extent of 10°, the difference in their expansibilities will vary to the extent of ·004; whilst if in one θ_0 be 4727° C. and the value of θ_0 in the other vary to the extent of 500°, the difference in their expansibilities will vary to the extent of ·00065. These three reasons, viz. (1) smallness of dissociation, (2) constancy of expansibility, (3) near equality of expansibilities of different gases, lead us to believe that in the case of the permanent gases θ_0 is large compared with ordinary temperatures. Still there may be gases whose θ_0 is small. It is possible that mercury vapour is such a gas, and that this accounts for the fact that its molecules are monatomic, viz. that the temperature at which it exists is a large multiple of its θ_0, and that therefore the dissociation is large.

19. In considering the effect of dissociation on the specific heats of gases, we may not treat them as a mixture of two whose proportions remain constant; for in raising the temperature, not only is work done in increasing the kinetic energy of translation and internal motion, but also in destroying a certain proportion of molecules. Let, as before, x be the number of molecules, $2y$ the number of atoms present when the temperature is θ. Let λ_1, λ_2 be the ratios of the whole energy to that of translation in molecules and free atoms respectively. Then we have, if Q is the quantity of energy present in unit of volume, and I the potential energy of combination of a single molecule,

$$Q = \tfrac{1}{2}2mv_1^2 . \lambda_1 x + \tfrac{1}{2}mv_2^2 . 2\lambda_2 y + Ix$$
$$= (\lambda_1 x + 2\lambda_2 y)\lambda\theta + Ix.$$

Let a quantity δQ of heat be applied, the volume being constant. Then

$$\delta Q = \left\{ \left(\lambda_1 \frac{dx}{d\theta} + 2\lambda_2 \frac{dy}{d\theta} \right)\lambda\theta + (\lambda_1 x + 2\lambda_2 y)\lambda + I\frac{dx}{d\theta} \right\} d\theta$$
$$= cd\theta,$$

where c is the specific heat at constant volume. Also

$$2(x+y) = N ;$$
$$\therefore \frac{dx}{d\theta} + \frac{dy}{d\theta} = 0.$$

And

$$c = \left\{ (-\lambda_1 + 2\lambda_2)\theta\frac{dy}{d\theta} + \lambda_1 x + 2\lambda_2 y - \frac{I}{\lambda}\frac{dy}{d\theta} \right\} \lambda ;$$

and if c' is the specific heat at constant pressure,

$$c' = c + p\frac{dv}{d\theta}.$$

Now $p = \dfrac{N}{3}(1+\zeta)\lambda\theta.$

If V be the volume occupied by the gas and N′ the number of atoms in it,

$$N = \frac{N'}{V}.$$

Hence

$$pV = \lambda\frac{N'}{3}(1+\zeta)\theta ;$$
$$\therefore \ p\frac{dV}{d\theta} = \lambda\frac{N'}{3}\left\{ 1 + \zeta + \theta \ \frac{d\zeta}{d\theta} \right\},$$

where p is constant;

$$\therefore \quad c' = c + \frac{\lambda N}{3}\left(1 + \zeta + \theta\,\frac{d\zeta}{d\theta}\right),$$

$$\frac{c'}{c} = 1 + \frac{N}{3}\,\frac{\theta\,\dfrac{d\zeta}{d\theta} + 1 + \zeta}{(2\lambda_2 - \lambda_1)\theta\dfrac{dy}{d\theta} + \lambda_1 x + 2\lambda_2 y - \dfrac{I}{\lambda}\dfrac{dy}{d\theta}}$$

$$= 1 + \frac{2}{3}\,\frac{\theta\,\dfrac{d\zeta}{d\theta} + 1 + \zeta}{(2\lambda_2 - \lambda_1)\theta\dfrac{d\zeta}{d\theta} + \lambda_1 + (2\lambda_2 - \lambda_1)\zeta - \dfrac{I}{\lambda}\dfrac{d\zeta}{d\theta}}.$$

In the above we have neglected the variation of the potential energy of molecules except in the case where they are broken up. The above formula is greatly simplified if we consider, as is highly probable, that a molecule has twice the number of degrees of freedom of a free atom—in other words, $2\lambda_2 = \lambda_1$; we then get

$$\frac{c'}{c} = 1 + \frac{2}{3}\,\frac{1 + \zeta + \theta\,\dfrac{d\zeta}{d\theta}}{\lambda_1 - \dfrac{I}{\lambda}\dfrac{d\zeta}{d\theta}}.$$

20. If the atoms be smooth and spherical (as has been supposed throughout the present paper), any internal energy an atom may have must be unaffected by change of temperature; and in this case $\lambda_1 = 2$.

The blow just sufficient to break up a molecule is c; if this acted in the most favourable manner, the work done in separating a molecule would be $\dfrac{c^2}{2m} = 2\lambda\theta_0$. Hence I is $< 2\lambda\theta_0$. If we put $I = 2\lambda\theta_0$,

$$\frac{c'}{c} = 1 + \frac{2}{3}\,\frac{1 + \zeta + \theta\,\dfrac{d}{d\theta}}{2 - 2\theta_0}$$

$$= 1 + \frac{1}{3}\,\frac{1 + \zeta + \theta\,\dfrac{d}{d\theta}}{1 - \theta_0\dfrac{d}{d\theta}}.$$

If θ_0 is small and about $100°$, *i. e.* $-173°$ C., we find from

Table I. that, at a temperature of about 70° C.,

$$1 + \zeta = 1{\cdot}671, \quad \frac{d\zeta}{d\theta} = {\cdot}00048, \quad \theta = 350\,;$$

whence it follows that

$$\frac{c'}{c} = 1{\cdot}609.$$

If θ_0 is large and about 5000° or 4727° C., Table II. gives us for a temperature of 70°

$$1 + \zeta = 1{\cdot}0028, \quad \frac{d\zeta}{d\theta} = {\cdot}00004,$$

whence

$$\frac{c'}{c} = 1{\cdot}423.$$

In the above we have confessedly taken I too large, as we showed that $2\lambda\theta_0$ was its superior limit; if we were to take $I = 2\lambda\theta_0 \times {\cdot}4$, we should get

$$\frac{c'}{c} = 1{\cdot}403.$$

The value of $\dfrac{c'}{c}$ for the permanent gases is about $1{\cdot}408$.

Not only does this result confirm our former conclusion that θ_0 is very large, but it adds largely to the probability of the general theory here set forth, since the great difficulty of the kinetic theory of gases hitherto has been to explain the value of $\dfrac{c'}{c}$ for the permanent gases whose molecules are diatomic. Mercury vapour which has simple molecules, or is monatomic, and which therefore has no dissociation, has the value given by the ordinary theory, as has been shown recently by Messrs. Kundt and Warburg.

21. To satisfy experience, it is further necessary to show that c does not vary much within the ordinary range of temperature. We have

$$c = \frac{\lambda \mathrm{N}}{2} \left\{ (2\lambda_2 - \lambda_1)\left(\theta\,\frac{d\zeta}{d\theta} + \zeta\right) + \lambda_1 - \frac{\mathrm{I}}{\lambda}\,\frac{d\zeta}{d\theta} \right\}$$

$$= \frac{\lambda \mathrm{N}}{2} \left\{ \lambda_1 - 2\kappa\theta_0\,\frac{d\zeta}{d\theta} \right\},$$

where κ is a fraction. Hence

$$\frac{dc}{d\theta} = -\lambda\kappa\mathrm{N}\theta_0\,\frac{d^2\zeta}{d\theta^2}.$$

Now $\dfrac{d\zeta}{d\theta}$ is throughout very small, and varies very slowly,

so that $\dfrac{d^2\zeta}{d\theta^2}$ is extremely small.   In fact, from Table II. we see

that at $100°$ C. $\dfrac{d^2\zeta}{d\theta^2}$ is somewhere about ·00000048.

<div align="center">[To be continued.]</div>

LVI. *Notes on the Theory of Sound.*   *By* R. H. M.
    Bosanquet, *Fellow of St. John's College, Oxford.*

<div align="center">[Continued from p. 349.]</div>

<div align="center">4. *On Combined Wave-systems.*</div>

THE principle that a stream of sound may be regarded as a flow of energy, which cannot of itself increase or diminish in quantity, enables us to deal with certain simple cases of combined wave-systems.   I restrict myself for the present to the case of plane waves.

Prop. I.—If two equal and similar pendulum wave-systems, travelling in opposite directions, meet in air, they form a stationary wave which will carry the whole energy of both.

Let

$$y_1 = a \sin \frac{2\pi}{\lambda} (vt - x),$$

$$y_2 = a \sin \frac{2\pi}{\lambda} (vt + x)$$

be the two equal systems travelling opposite ways; the more general equations can always be reduced to this form by suitable choice of the origins of space and time.

Then

$$\mathrm{Y} = y_1 + y_2 = 2a \sin \frac{2\pi vt}{\lambda} \cos \frac{2\pi}{\lambda} x,$$

$$\frac{d\mathrm{Y}}{dt} = \frac{4a\pi v}{\lambda} \cos \frac{2\pi vt}{\lambda} \cos \frac{2\pi}{\lambda} x,$$

$$\frac{d\mathrm{Y}}{dx} = - \frac{4\pi a}{\lambda} \sin \frac{2\pi vt}{\lambda} \sin \frac{2\pi}{\lambda} x.$$

When $t = 0$, $\mathrm{Y} = 0$ and $\dfrac{d\mathrm{Y}}{dx} = 0$, or there is no displacement and no pressure anywhere along the stationary wave, but there is a maximum velocity,

$$\frac{d\mathrm{Y}}{dt} = \frac{4a\pi v}{\lambda} \cos \frac{2\pi x}{\lambda};$$

when $t = \frac{\tau}{4}$ (quarter period), there is no velocity anywhere, but there is a maximum displacement,

$$Y = 2a \cos \frac{2\pi}{\lambda} x,$$

and a maximum pressure,

$$1 \cdot 4 \, \Pi \cdot \frac{4\pi a}{\lambda} \sin \frac{2\pi}{\lambda} x.$$

Consider a tube having the length of the velocity of sound, in which such a stationary wave is maintained by the transmission from the opposite ends of the equal and opposite wave-systems. The energy in the tube is to be found.

We have shown that the form of energy alternates entirely between potential and kinetic. In either form it is identical in distribution and magnitude with the kinetic or potential energy of an ordinary wave of transmission in air of the same amplitude; so that there is only one quantity in the stationary wave where there would be two in the ordinary wave of transmission. The energy in the tube of length v is therefore

$$1 \cdot 4 \, \Pi v \left(\frac{\pi A}{\lambda} \right)^2, \quad (A = 2a,)$$

or half that in a wave of transmission of amplitude A. And this may be written

$$2 \left\{ 2 \times 1 \cdot 4 \, \Pi v \left(\frac{\pi a}{\lambda} \right)^2 \right\},$$

which is twice the energy of either single stream, or the sum of both.

Cor.—Putting $x = 0$, we have the conditions of a loop surface; and it follows without difficulty that, If a disk of air oscillate with maximum velocity V and without change of density, the energy per second through the disk consists of two equal and opposite streams each $= \frac{\rho v}{8} V^2$ per second, making a total transfer of $\frac{\rho v}{4} V^2$ per second through the disk. (Section unity.)

Prop. II.—If any two wave-systems of the same wavelength (pitch, or periodic time) meet in air, travelling in opposite directions, their direct superposition carries the energy of both.

Let

$$y_1 = a \sin \frac{2\pi}{\lambda}(vt - x),$$

$$y_2 = b \sin \frac{2\pi}{\lambda}(vt + x),$$

$$Y = y_1 + y_2,$$

$$\frac{dY}{dt} = \frac{2\pi v}{\lambda}\left(b \cos \frac{2\pi}{\lambda}(vt + x) + a \cos \frac{2\pi}{\lambda}(vt - x)\right),$$

$$\frac{dY}{dx} = \frac{2\pi}{\lambda}\left(b \cos \frac{2\pi}{\lambda}(vt + x) - a \cos \frac{2\pi}{\lambda}(vt - x)\right).$$

The total energy in any disk dx, estimated as kinetic + potential, is (Phil. Mag. [4] vol. xlv. p. 174)

$$\frac{\rho dx}{2}\left\{\left(\frac{dY}{dt}\right)^2 + v^2\left(\frac{dY}{dx}\right)^2\right\}.$$

The coefficient of $2ab$ disappears from this expression, leaving only the terms involving a^2 and b^2, which give on integration the same value of the energy per second as the sum of the values for the component streams. This, of course, includes the result of prop. I.

Prop. III.—If two equal and similar pendulum wave-systems, in the same phase and travelling in the same direction, join each other in air, they cannot be superposed without alteration.

Let each of the wave-systems have an amplitude a; then, if they are simply superposed, the combined stream has an amplitude $2a$, and the energy per second carried by the combined stream is four times that of each of the single streams, or twice the sum of the energy of the two streams together. Hence the energy required for the superposition is twice the total energy supplied, and simple superposition cannot take place.

Cor.—Reflexion will generally take place at the point of junction.

Prop. IV.—Two equal and similar wave-systems, in the same phase and travelling in the same direction, join each other in air; to determine the transmission and reflexion.

Let A be the amplitude of each of the original streams,

    a that of the reflected stream,

    b that of the transmitted stream.

At the common surface,

$$2A + a = b;$$

and equating values of energy per second,

$$2A^2 = a^2 + b^2;$$

eliminating A, we find

$$a + b = 0.$$

The reflected and transmitted streams are equal; and each of them is equal to either of the original streams.

This proposition finds an application in some cases of interference. The circumstances supposed here cannot, however, be realized in an accurate manner physically; for the two original streams can only be kept apart before their junction by travelling in different channels. In this case there are two reflected streams, and the solution is different.

Prop. V.—Two equal and similar wave-systems, in the same phase and travelling in the same direction by two separate channels, join each other by the two channels uniting into one, of the same size as either; to determine the transmission and reflexion.

At the common surface at the entry to the channel of union,

$$2(A + a) = b,$$

$$2(A^2 - a^2) = b^2 = 4(A + a)^2,$$

whence

$$A^2 + 4Aa + 3a^2 = 0,$$

or

$$(A + a)(A + 3a) = 0.$$

$A + a = 0$ involves $b = 0$ and is inadmissible;

$$\therefore \quad a = -\frac{A}{3}, \quad b = \tfrac{4}{3}A;$$

and of the total energy incident per second, $\tfrac{8}{9}$ is transmitted in the combined vibration, the remaining $\tfrac{1}{9}$ being reflected back along the paths of the incident wave-systems.

These circumstances may be realized in the case where the wave-length is great compared with all the dimensions, so that any difference of direction between the original and united channels becomes immaterial.

Prop. VI.—In the general case, where two systems of the same wave-length join in air, to determine the reflexion and transmission, considering one channel, *i. e.* under the circumstances of prop. IV.

Let A, B be the amplitudes of the incident wave-systems,
  γ the difference of phase,
  a the amplitude of the reflected system,
  b the amplitude of the transmitted system.

Then
$$y_1 = \text{A} \sin \frac{2\pi}{\lambda} (vt - x),$$

$$y_2 = \text{B} \sin \left\{ \frac{2\pi}{\lambda} (vt - x) + \gamma \right\},$$

$$y_1 + y_2 = \text{C} \sin \left\{ \frac{2\pi}{\lambda} (vt - x) + \text{D} \right\},$$

where
$$\text{C}^2 = \text{A}^2 + \text{B}^2 + 2\text{AB} \cos \gamma.$$

It is clear that this combined vibration cannot generally be transmitted unaltered; for the energy per second would be altered by a term depending on $2\text{AB} \cos \gamma$.

Proceeding as before, we have, equating the values of the amplitude at the common surface,
$$\text{C} + a = b,$$
whence
$$\text{A}^2 + \text{B}^2 + 2\text{AB} \cos \gamma = (b - a)^2,$$
and, equating values of energy per second,
$$\text{A}^2 + \text{B}^2 = a^2 + b^2;$$
whence
$$\text{A}^2 + \text{B}^2 - 2\text{AB} \cos \gamma = (a + b)^2.$$
Let
$$\text{A}^2 + \text{B}^2 - 2\text{AB} \cos \gamma = \text{C}'^2,$$
then
$$\text{C} = b - a,$$
$$\text{C}' = b + a,$$
$$\frac{\text{C} - \text{C}'}{2} = -a,$$
$$\frac{\text{C} + \text{C}'}{2} = b;$$

whence, if we draw from a point O two radii equal to A, B respectively, enclosing the angle γ, and complete the parallelogram of which they form two sides, half the difference of the diagonals is the reflected amplitude a, and half the sum of the diagonals is the transmitted amplitude b. The diagonal through O is of course C, the amplitude derived by geometrical superposition.

The ratios of energy in the above case are:—
$$\frac{\text{transmitted}}{\text{incident}} = \frac{b^2}{\text{A}^2 + \text{B}^2}$$

$$= \tfrac{1}{2} \left(1 + \sqrt{1 - \frac{4\text{A}^2\text{B}^2 \cos^2 \gamma}{(\text{A}^2 + \text{B}^2)^2}} \right);$$

$$\frac{\text{reflected}}{\text{incident}} = \frac{a^2}{A^2 + B^2}$$

$$= \tfrac{1}{2}\left(1 - \sqrt{1 - \frac{4A^2B^2\cos^2\gamma}{(A^2+B^2)^2}}\right).$$

The transmission is complete only when $\gamma = \dfrac{\pi}{2}$, or the incident wave-systems differ in phase by quarter of a period.

When the phase-difference is either zero or half a period, the transmission is a minimum and the reflexion a maximum ; the values are :—

$$\text{Transmission} = \frac{A^2}{A^2+B^2},$$

$$\text{Reflexion} = \frac{B^2}{A^2+B^2},$$

where $A > B$.

(For $C - C' = -2a$, and $C + C' = 2b$; and C, C' are essentially positive quantities.)

Prop. VII. In the general case where two systems of the same wave-length, travelling in the same direction by two separate channels, join each other by the two channels uniting into one, of the same size as either ; to determine the transmission and reflexion, *i. e.* under the circumstances of prop. V.

Following the notation of the last proposition, we have for the common amplitude at the entry of the channel of union,

$$C + 2a = b, \quad \ldots \quad \ldots \quad \ldots \quad \text{(i)}$$

whence

$$A^2 + B^2 + 2AB\cos\gamma = (b - 2a)^2 = C^2,$$

and, equating values of energy per second,

$$A^2 + B^2 = 2a^2 + b^2, \quad \ldots \quad \ldots \quad \text{(ii)}$$

whence

$$A^2 + B^2 - 2AB\cos\gamma = C'^2$$
$$= b^2 + 4ab.$$

From this and (i),

$$3b^2 - 2bC - C'^2 = 0,$$

$$b = \frac{C \pm \sqrt{C^2 + 3C'^2}}{3};$$

and from (i),

$$a = \frac{b - C}{2}.$$

The ratios of energy are :—

$$\frac{\text{transmitted}}{\text{incident}} = \frac{b^2}{A^2 + B^2}$$

$$= \frac{1}{9}\left\{ 5 + \frac{-2AB\cos\gamma \pm 4C\sqrt{A^2 + B^2 - AB\cos\gamma}}{A^2 + B^2} \right\};$$

$$\frac{\text{single reflected stream}}{\text{incident}} = \frac{a^2}{A^2 + B^2}$$

$$= \frac{1}{9}\left\{ 2 + \frac{AB\cos\gamma \mp 2C\sqrt{A^2 + B^2 - AB\cos\gamma}}{A^2 + B^2} \right\}.$$

If $\gamma = 0$, or $\cos\gamma = 1$, and $A = B$, we have the case of prop. V. This determines the employment of the upper signs before the roots in the present results, in which case these results coincide with those of prop. V., giving $\frac{8}{9}$ of the whole transmitted, and the remaining $\frac{1}{9}$ divided between the reflected streams. If $\cos\gamma = 0$, or the component systems differ in phase by $\frac{1}{4}$ of a period, the whole energy is transmitted, or the geometrical composition takes place unaltered.   This is immediately seen from the vanishing of the term $(2AB\cos\gamma)$, by which C^2 differs from $A^2 + B^2$.

In all these discussions it has been assumed that the incident wave-systems were not subject to constraint, which would prevent them from yielding to the reflected impulse ; and in cases of interference in air constraint is frequently absent.   But where the two ·wave-systems are supplied, say, by tuning-forks close to the point of meeting, the forks are capable of maintaining their movement unchanged and acting as a constraint. Under these circumstances more work is done by the sources, and the geometrical composition of the systems is maintained.

In the next note I propose to apply the principle of the flow of energy to the divergence of sound in air.

LVII. *Experimental Researches on the supposed Diathermancy of Rock-Salt.   By* John Rushton Harrison [*].

[Plate III.]

IT is almost universally accepted that pure rock-salt transmits more than 92 per cent. of the total radiation from heated bodies.   I would ask permission to glance for a moment at the mode of experiment by which Melloni arrived at this conclusion.

A thermopile is placed at a distance from the source of heat, the radiation from which causes a deflection of the galvano-meter-needle. This arrangement completed, the substances to be examined are introduced between the source and the pile, their different powers of absorption and transmission being determined by the different values of the deflections. Taking a single instance, it is assumed that a plate of ice $\frac{1}{10}$ of an inch thick absorbs all the incident radiation from copper heated to 400° C.; while a rock-salt plate of the same thickness *transmits* 92·3 per cent. of the total radiation from the same source of heat. That the ice *does* absorb the heat is proved by the liquefaction of the substance; but that 92·3 per cent. of the total radiation *passes through* the salt is not, I think, equally certain; in fact the experiment points, not to diathermancy, but to the unequal *absorptive* powers of the different substances examined.

In 1869* Professor Magnus endeavoured to account for the diathermancy of rock-salt by saying :—"The great diather-mancy of rock-salt does not depend on a small absorbing-power for different kinds of heat, but upon the circumstance that it emits only one kind of heat, and only absorbs this one, and that almost all other bodies at a temperature of 150° C. emit heat which only contains a small portion, or none at all, of the heat which rock-salt emits." There will be little doubt that this conclusion is based upon the doctrine of periods, although the result of another experiment with fluor-spar, noticed in the same paper, is *fatal* to that conclusion.

It is less, I think, from an experimental point of view than from unsuccessful attempts to explain *why* a solid substance should be diathermanous, that any one would be led to doubt the value of Melloni's conclusions; and here I would state that the experiments recorded in this paper, by which opposite results have been obtained, were not originally suggested by any apparent inefficiency in the mode of experiment adopted by this great philosopher.

The apparatus by which these results have been obtained is as follows:—Two thermometers, each 3 inches long, with bulbs $\frac{1}{10}$ of an inch diameter, registering from 0° to 200° C. One of these thermometers is enclosed in a rock-salt case $3\frac{1}{2}$ inches long, bored out so that the bulb stood at a distance of $\frac{1}{10}$ of an inch from the salt; the scale of the thermometer is plainly visible through the salt case, which consists of two pieces, as shown in fig. 1, afterwards cemented together with a thin film of transparent glue. The sides of the case are $\frac{1}{10}$ of an inch

* Philosophical Magazine for November 1869.

thick; a few threads of unspun silk wound round the head of the thermometer-tube hold it in position. This I will call the "enclosed" thermometer, and the other the "naked" one; both thermometers are fixed to one cork and placed in a glass tube. The whole apparatus is seen in fig. 2. C is the cork with thermometers attached, the bulbs of which are at a distance of one inch from each other; T, a glass tube 12 inches long and $1\frac{1}{2}$ inch internal diameter containing them; V, a glass vessel 8 inches long and 4 inches internal diameter, fitted with a cork to admit the tube T; this vessel is filled with water at the boiling-temperature; t, a tube to convey the steam given off by the water to condenser K. The mode of performing the experiment is as follows:—

The thermometers being in their places, the tube containing them is placed in a freezing-mixture and allowed to remain till both thermometers register 0° C. The water in the vessel V is heated by the aid of a spirit-lamp which is withdrawn when the water boils; when ebullition has ceased the tube is taken from the freezing-mixture and quickly passed into the vessel V, and the rise of both thermometers noted from minute to minute. When the enclosed thermometer has reached its maximum temperature, the tube is then withdrawn, placed in the freezing-mixture, and the descent from minute to minute noted. The result of the experiment is seen in the following Table:—

Registration of both thermometers at commencement of experiment, 0° C.

Naked thermometer.				Enclosed thermometer.			
After the lapse of	1 minute, 35°;	gain in temp.	35°	After the lapse of	1 minute, 9°;	gain in temp.	9°
,,	2 ,, 55	,,	20	,,	2 ,, 20	,,	11
,,	3 ,, 64	,,	9	,,	3 ,, 28	,,	8
,,	4 ,, 69	,,	5	,,	4 ,, 35	,,	7
,,	5 ,, 70	,,	1	,,	5 ,, 40	,,	5
,,	6 ,, 70	,,	0	,,	6 ,, 45	,,	5
,,	7 ,, 71 (max.)	,,	1	,,	7 ,, 49	,,	4
,,	8 ,, 71	,,	0	,,	8 ,, 52	,,	3
,,	9 ,, 71	,,	0	,,	9 ,, 55	,,	3
,,	10 ,, 70	Loss	1	,,	10 ,, 58	,,	3
,,	11 ,, 70	,,	0	,,	11 ,, 60	,,	2
,,	12 ,, 69	,,	1	,,	12 ,, 61	,,	1
,,	13 ,, 69	,,	0	,,	13 ,, 62	,,	1
,,	14 ,, 68	,,	1	,,	14 ,, 63	,,	1
,,	15 ,, 67	,,	1	,,	15 ,, 63	,,	0
,,	16 ,, 66	,,	1	,,	16 ,, 63	,,	0
,,	17 ,, 65	,,	1	,,	17 ,, 64(max.)	,,	1
,,	18 ,, 65	,,	0	,,	18 ,, 63	Loss	1
,,	19 ,, 64	,,	1	,,	19 ,, 63	,,	0
,,	20 ,, 64	,,	0	,,	20 ,, 63	,,	0
,,	21 ,, 62	,,	2	,,	21 ,, 62	,,	1

The tube containing the thermometers was then placed in the freezing-mixture.

Naked thermometer.			gain in temp.		*Enclosed thermometer.*			gain in temp.	
After the lapse of	22 minutes, 35°;			27°	After the lapse of	22 minutes, 52°;			10°
,,	23	,,	16	,, 19	,,	23	,,	40	,, 12
,,	24	,,	8	,, 8	,,	24	,,	30	,, 10
,,	25	,,	5	,, 3	,,	25	,,	20	,, 10
,,	26	,,	3	,, 2	,,	26	,,	17	,, 3
,,	27	,,	0	,, 3	,,	27	,,	15	,, 2
After the lapse of	5	,,	0	0	After the lapse of	5	,,	7	,, 8
,,	5	,,	0	0	,,	5	,,	3	,, 4
,,	5	,,	0	0	,,	5	,,	1	,, 2
,,	5	,,	0	0	,,	5	,,	0	,, 1

It will be noticed that the naked thermometer reached its maximum temperature 71° C. in seven minutes, at which time the enclosed thermometer registered 49° C. The former remained stationary at 71° C. for two minutes, and then slowly descended; after the lapse of ten minutes from the time it first reached its maximum, and seventeen minutes from the commencement of the experiment, it had fallen 6° C. and now registered 65° C. During this time the enclosed thermometer steadily increased in temperature, and now registered its maximum (64° C.), it having risen 15° C., while the naked thermometer had fallen 6° C. This reverse action proves beyond doubt that the heat incident on the bulb of the enclosed thermometer had been radiated from the salt as an independent source, and not diathermanously transmitted.

The following experiment, though void of numerical value, is still, I think, interesting, as a different source of heat is employed. The apparatus used is shown in fig. 3 : T is a test-tube with foot, 8 inches long and $1\frac{1}{2}$ inch internal diameter; t, a second tube passed into the first and held in position by means of a cork, c (this inner tube is $\frac{3}{4}$ of an inch internal diameter, and stands half an inch higher than the outer one); P, a pivot fixed in the inner tube to support the source of heat —a round bar of hot copper 2 inches long and half an inch cross section ; C, a movable German-silver cap open at both ends and polished on both sides ; its smallest circumference passes into the inner tube, and its larger circumference passes into the outer one. One surface of a piece of white blotting-paper is coated with a thin layer of melted white wax, care being taken that the wax does not penetrate *through* ; when this layer is dry, others are applied till the texture of the paper is well filled.

The experiment is performed as follows:—

The outer tube being filled with water, the bar of copper heated to a dark red is placed in the inner tube, the cap adjusted, and a rock-salt plate $\frac{1}{20}$ of an inch thick shaped thus
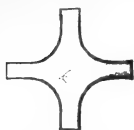
placed on the top of the cap, the blotting-paper

with its waxed surface downwards brought immediately over but not touching the salt: the heat ascending melts the wax, and a well-defined outline of the rock-salt plate is produced on

the upper surface of the blotting-paper, thus
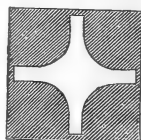
If the experiment be performed without the rock-salt plate, the melted wax first shows itself as a dark spot in the centre of the blotting-paper, and then spreads towards the edges. Different shaped plates were used with similar results. In performing the experiment care must be taken that the heated copper stands fairly perpendicular under the centre of the cap.

LVIII. *On the Thermal Phenomena of the Galvanic Pile, and Electromotive Forces.* By E. EDLUND[*].

§ I.

1. FROM the experiments which have been instituted for the purpose of studying the thermal phenomena of the galvanic pile and its conductors the conclusion has been drawn, that the heat which arises in consequence of the passage of the current through the entire conduction (including the pile itself) during a certain time is exactly equal in quantity to that which is produced in the pile by chemical processes during the same time,—that is to say, provided that the current performs no external work (for example, induction, chemical decomposition, &c.); and among the processes mentioned, only those must be understood which are primary and in direct connexion with the formation of the current. In the following, to distinguish these two quantities of heat from one another, we will name that which is occasioned by the passage of the current through the conductors the gal-

vanic, and that which arises from the chemical processes in the
pile the chemical heat. Then it has been inferred, from the
experiments which have been made, that, under the presuppo-
sition mentioned, the chemical is equal in amount to the gal-
vanic heat.   Calling the galvanic heat gw, according to Joule's
law $gw = \mathrm{M}i^2lt$, where M is a constant, i denotes the current-
intensity, l the resistance of the conduction and the pile toge-
ther, and t the time during which the current is in action.
Therefore, if E denotes the electromotive force of the pile, we
can also write $gw = \mathrm{M}\mathrm{E}it$, from which, in consequence of the
inference drawn, we obtain $kw = \mathrm{M}\mathrm{E}it$, if kw signifies the heat
evolved by the primary chemical processes in the pile.   If n
denotes the number of chemical equivalents decomposed by the
action of the current at the positive-pole plate of the pile, ac-
cording to the law of electrolysis $n = mit$, m denoting a constant
which is independent of the nature of the electrolytic liquid.
Hence we conclude that $kw = \dfrac{\mathrm{M}n}{m}\mathrm{E}$, and therefore that, for
one equivalent, $kw = \dfrac{\mathrm{M}}{m}\mathrm{E}$, from which it follows that the quan-
tity of heat which is produced in the pile by the primary che-
mical processes while one equivalent is decomposed at the
positive pole is a measure of the electromotive force of the
pile.

If the galvanic is in reality precisely as great as the primary
chemical heat, we may consequently say that the whole effi-
cacy of the current consists only in this, that it conducts the
chemical heat to all parts of the closed circuit, and deposits at
each place exactly as much as corresponds to the resistance at
the same place—although of course it is very difficult to form
a clear conception of the actual physical processes that take
place in this conduction.   If by direct measurement of the
heat produced in the pile itself it were to be found that its
amount is greater than that of the galvanic heat occasioned
by the passage of the current, or, in other words, exceeds
the heat which the current calls forth in a metallic conduc-
tor whose resistance is equal to that of the pile, we should
have to admit that this excess was derived from the secon-
dary processes which may take place in the pile and have
nothing in common with the formation of the current.   In
this way also it has been attempted to explain such surplus
heat in the cases in which it has been observed :—If a chemi-
cal-decomposition cell or a voltameter be inserted in the circuit,
so that the current has opportunity to decompose water, for
example between platinum poles, then, according to the way
of regarding it now presented, all the primary chemical heat

arising in the pile cannot pass over into galvanic heat, but a part of it is expended for the mechanical work necessary for generating the electromotive forces of polarization and the chemical decomposition in the cell. We can imagine this brought about by the store of chemical heat requisite for this work being carried by the current from the pile into the decomposition-cell, where it is employed for the purpose mentioned. Consequently no other change of temperature can arise in the decomposition-cell than that which is occasioned by the passage of the current through the electrolytic liquid. The heat generated in the decomposition-cell must therefore be equal to that which is produced when the current passes through a metallic conductor the resistance of which is equal to that of the liquid. Now, as by direct measurement the quantity of heat produced in the decomposition-cell has been found greater than the galvanic, the cause of this has been sought in the secondary chemical processes which may take place there and are independent of the current.

2. I have already, some years since *, given another explanation of the thermal phenomena in question. It was, in brief, the following:—If the current does no external work, its total action consists in calling forth heat in the conductor through which it passes. After the current has ceased, no other products of the activity of the pile are found but the chemical changes in the pile and the heat which has arisen, partly in the pile itself, and partly in the conductors. It is evident, however, that the amount of this heat must be equivalent to the chemical changes; that is, in other words, the quantity of heat generated must be exactly equal to that which would have resulted from the same chemical changes if no current had taken place; for otherwise either chemical work or heat would have been obtained out of nothing. The current has therefore, upon the whole, generated no heat at all; its total heat-production is equal to *nil*. But we know that the current does a certain amount of mechanical work in order to overcome the resistance of the galvanic conduction; and this work changes into heat. Therefore the current brings forth in the conduction an actual production of heat. But, because the total heat-production of the current must be equal to *nil*, this can only happen through a consumption of heat occurring at some place or other in the conduction; and of course the place can be no other than that where the electromotive force has its seat. We consequently arrive at the result that, in order to produce the cur-

* *Öfversigt af K. Vetenskaps Akademiens Förhandlingar*, 1869; Pogg. *Ann.* vol. cxxxvii. p. 174.

rent, the electromotive force consumes a quantity of heat exactly equal to the quantity generated by the current in overcoming the resistance of the galvanic conduction. The heat-consumption of the electromotive force is accordingly equal to gw; yet it does not hence follow that it is not also equal to kw, or that gw and kw have not the same magnitude.

If only a single electromotor is inserted in the closed circuit, keeping the same notation as before, we have

$$gw = \mathrm{M}i^2lt = \mathrm{M}\mathrm{E}it;$$

therefore in unit time a quantity of heat is consumed which is proportional to the product of the electromotive force and the intensity of the current. Thus, during the solution of one equivalent of zinc, the total amount of heat gw consumed by the electromotor $= \dfrac{\mathrm{M}}{m}\,\mathrm{E}$. This holds, even if l be changed—that is, even if the current-intensity be increased or diminished. Are two electromotors E and E′ acting in the same direction? then in unit time the total heat-consumption in both must be $\mathrm{M}(\mathrm{E}+\mathrm{E}')i_{\prime}$, if $i_{\prime}$ denotes the intensity of the current produced; hence, evidently, $\mathrm{M}\mathrm{E}i_{\prime}$ is consumed in the former, and $\mathrm{M}\mathrm{E}'i_{\prime}$ in the latter. When E is greater than E′ and the one electromotor acts in the opposite direction against the other, the total quantity of heat consumed becomes $\mathrm{M}(\mathrm{E}-\mathrm{E}')i_{\prime\prime}$, if $i_{\prime\prime}$ denotes the current-intensity. In the first electromotor the quantity $\mathrm{M}\mathrm{E}i_{\prime}$ of heat is now consumed; but this is greater than the total quantity generated by the current in consequence of the galvanic resistance. In the other electromotor, therefore, a quantity of heat equal to $\mathrm{M}\mathrm{E}'i_{\prime\prime}$ must be generated. Consequently, when the current traverses the electromotor in the same direction in which the electromotive force acts, a quantity of heat is consumed which is proportional to the product of the electromotive force and the current-intensity; but if the current goes in the contrary direction, just as great a quantity of heat is produced instead[*].

From this it is evident that these two ways of considering the subject agree in one respect, namely that, according to both, the sum of the heat which the current produces upon the whole is equal to *nil*; but in the one case the heat produced in the pile by the chemical processes is regarded as conveyed to the different parts of the circuit; while in the other heat is supposed to be generated by the current everywhere in the circuit; yet the total quantity of heat produced is equal

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[*] The unitarian view of the nature of electricity leads direct to the same result. See "Théorie des phénomènes électriques," p. 45 (*K. Vetenskaps Ak. Handl.* Bd. xii. No. 8: also Brockhaus, Leipzig).

to that which is consumed by the electromotive force. In other respects the two views lead to divergent results : for example, according to one view the quantity of the primary chemical heat is equal to that of the galvanic, wherefore the former also gives a measure of the electromotive force ; according to the other these two quantities may be different, and consequently the primary chemical heat cannot serve as a measure for the electromotive force, &c.

In order to determine which of these two views accords best with experience, we will more closely consider the experiments which have been instituted for the purpose of studying the thermal phenomena of the pile and of the current. For brevity, we will name the first-cited method of consideration No 1, and that proposed by me No. 2.

3. Favre * has endeavoured, by direct experiments, to answer the question, Is the whole of the galvanic heat which arises in the circuit derived merely from that which is generated by the chemical processes ? For this he made use of a mercury calorimeter with two muffles situated close to one another, of the same nature as the calorimeter which Favre and Silbermann had previously employed in their determinations of the heat developed in chemical processes. The pile he used consisted of a glass tube filled with water containing sulphuric acid, in which were placed the two pole-plates, amalgamated zinc and platinized copper (Smee's pile). It was closed with a fine platinum wire, which was of unequal length and thickness in different experiments. The hydrogen evolved in the pile was collected and measured. The experiment was first made in this way :—The pile was enclosed in one muffle, and the platinum wire in the other ; at the same time it must be remarked that the copper wires connecting the pole-plates with the ends of the platinum wire, and which were outside of the calorimeter, were so thick that no perceptible development of heat could take place in them. When, therefore, the experiment was thus arranged, the calorimeter indicated the total sum of the heat which was developed in the pile and the entire circuit while the current was in action ; and since the quantity of the hydrogen gas developed was at the same time known, it was easy to calculate what the sum of the heat would have been if the experiment had been continued until one equivalent of zinc in the pile was dissolved. Thereupon the experiment was varied by leaving the platinum wire outside of the calorimeter, in consequence of which the galvanic heat which arose in the wire had no effect upon the calorimeter. The

* *Annales de Chimie et de Physique*, (2) t. xl. p. 293 (1854).

difference between the indications of the calorimeter in the two cases was consequently equal to the galvanic heat which was developed in the platinum wire. The results of the different experiments will be seen from the following Table. Column a gives the length of the wire ; b, the deflection of the calorimeter in heat-units (the gram being taken as unit of weight) when both the pile and the platinum wire were enclosed in the calorimeter ; c, the corresponding deflection when the platinum wire was excluded from the calorimeter ; $b-c$, the difference between these two deflections, or the galvanic heat developed in the wire ; and gw the quantity, thence obtained by calculation, of the galvanic heat in the pile and the platinum wire together.

Diameter of the wire $=0\cdot265$ millim.

a.	b.	c.	$b-c$.	gw.
millim.				
25	18092	13127	4965	} 9652
50	18247	11690	6557	
100	18185	10439	7746	
200	18022	8992	9030	} 10820

In another series of experiments, in which a thinner wire was used, the following results were obtained :—

Diameter of the wire $=0\cdot175$ millim.

a.	b.	c.	$b-c$.	gw.
millim.				
50	18082	9955	8127	} 12040
50	18173	10101	8072	
100	18066	8381	9685	
Mean ...	18124	......	......	10837

As the preceding Tables show, the value of gw increases as the resistance becomes greater and therefore the current-intensity less.

According to Favre and Silbermann, there are developed,

Heat-units.

in the combination of one equivalent of zinc (33 grams) with oxygen . . } 42451

in the combination of the oxide with sulphuric acid . . . . } 10455

On the other hand, in the decomposition of one equivalent of water, } 34462 { are consumed.

Sum . . 18444

The mean, 18124, of the numbers found in the above experiments differs only about 300 heat-units from the last-mentioned sum ; wherefore the two may be regarded as equal. But according to both views, No. 1 and No. 2, this equality must exist ; and so we have no clue to enable us to judge in what way the galvanic current is produced ; whether we take No. 1 or No. 2 as the correct way of considering, in this respect we come to the same result. The calculation of the quantity of galvanic heat developed by the current in the entire circuit (a calculation not carried out by Favre in these experiments) shows that it amounts on the average to 10837 thermal units, not much more than the half of the chemical heat.

The circumstance that, as results from the above experiments, the galvanic heat gw developed by the current in the entire circuit increases when the resistance is increased and consequently the current-intensity is diminished, is confirmed by Favre's later experiments with Smee's pile [*]. In one of these experiments, the length of the platinum wire being shortened from 7000 to 250 millims., the galvanic heat was lessened from 18018 to 14424 thermal units ; the chemical heat he found somewhat greater than before, namely 19834 units. While the chemical heat, as we know beforehand, is constant and independent of the inserted resistance, the galvanic heat, on the contrary, became less when the resistance was diminished and consequently the intensity of the current was augmented. In all these experiments, however, the chemical was greater than the galvanic heat.

In the following experiments, on the contrary, the galvanic exceeded the chemical heat ; for there appeared in the pile an actual consumption of heat, so that its temperature sank on the passing of the current, instead of rising [†]. One of the pole-disks of the pile employed consisted of platinum, the other of zinc or cadmium ; and both were immersed in hydrochloric acid. The closed pile was first put into the calorimeter without any exterior resistance, by which a measure of the total chemical heat was obtained. Afterwards the pile was furnished with a considerable exterior resistance and then enclosed in the calorimeter, but so that the resistance was left outside. The chemical heat amounted, for the cadmium-platinum pile, to 7968 units, and for the zinc-platinum pile to 15899 ; but when the resistance was left outside of the calorimeter, there was observed, in the cadmium-platinum pile, a

[*] *Comptes Rendus*, t. xlvii. p. 599 (1858), and t. lxvii. p. 1012 (1868).
[†] Ibid. t. lxviii. p. 1300 (1869).

lowering of temperature 1288, and, in the zinc-platinum pile, 1051 thermal units.

Favre * has also, with the aid of the mercury calorimeter, determined how much chemical and galvanic heat is liberated in some piles of another construction during the solution of one equivalent of zinc †.

We will here take into consideration only the numerical values obtained by Favre for the piles of Daniell and Grove. If kw denote all the chemical, and gw all the galvanic heat, he obtained for Daniell's pile $kw = 25060$, $gw = 23993$, and consequently $kw - gw = 1067$ thermal units ; for Grove's pile $kw = 41490$, $gw = 46447$, and consequently $kw - gw = -4957$ units ; so that in Daniell's pile the chemical heat is only about 1000 units greater than the galvanic, while in Grove's pile the galvanic is greater than the chemical heat. Consequently, when this pile is closed with a conducting wire of great resistance, the pile itself is cooled during the passage of the current, while the conducting wire is heated.

We may add that Raoult also has determined by direct ex-

* *Comptes Rendus*, t. lxix. p. 34.
† The following was the method of observation here employed :—The pile to be investigated was enclosed in one muffle of the calorimeter. As the progress of the chemical processes could be determined with great accuracy by measuring the hydrogen which was evolved in the Smee's pile, such a pile was also placed in the calorimeter, and connected with the other pile, so that the current passed through both. The rheostat, which connected the two poles, was also enclosed in the calorimeter. Thus the calorimeter gave the chemical heat developed in both piles. The quantity of heat shown by the calorimeter while half an equivalent of hydrogen was being evolved in the Smee pile was now observed—that is, during the time that the chemical processes in the two piles together corresponded to one equivalent. When a Daniell's and a Smee's pile were placed in the calorimeter, 22447 heat-units were in this way obtained. The quantity of chemical heat for one equivalent of hydrogen, in the Smee's pile employed, was determined, by special trial, at 19834 thermal units, or 9917 for a half-equivalent. Consequently, if the quantity of chemical heat corresponding to 1 equivalent for the Daniell's pile be called x, we shall have

$$\frac{x}{2} + 9917 = 22447, \text{ whence } x = 25060.$$

A fresh experiment was then made, in which the Smee's pile and the rheostat resistance were taken out of the calorimeter, so that the Daniell's pile alone remained within. The outer resistance was so great, that the heating caused by the current in consequence of the resistance in the pile itself could be neglected. With this arrangement, the calorimeter now gave the difference between the chemical and the galvanic heat, or $gk - gw$. In this way was obtained, during the evolution of one equivalent of hydrogen in the Smee pile, $gk - gw = 1067$ thermal units. Subtracting this number from 25060, we get 23993, which denotes the galvanic heat of the Daniell pile. Favre proceeded in the same manner in his investigation of the other piles.

periments the galvanic heat of Daniell's pile, and found that it amounts to 23900 thermal units, which agrees closely with M. Favre's result[*].

If the difference which is almost always found between the quantities of the chemical and the galvanic heat is caused by the secondary chemical processes which may take place in the pile, we must assume, in accordance with what has been said above, that these processes bring forth in some piles a production, in others a consumption of heat. Favre thought at first that the galvanic heat was exactly equal in quantity to the chemical[†]. Subsequently he shared the view already expressed by others, that the cause of the difference in question in Smee's pile lay in this, that the hydrogen at the negative platinum disk was separated in the active state, or *in statu nascenti*. As afterwards the hydrogen leaves the platinum disk and escapes upward through the liquid, it passes over into its ordinary condition, in which process heat is liberated, which probably raises the temperature of the liquid, but does not augment the electromotive force[‡]. But the unexpected relation shown by the cadmium-platinum and zinc-platinum piles charged with hydrochloric acid finally convinced him that this also could not be the true explanation[§].

In order, therefore, to explain the difference in the ordinary Smee's pile, we must assume that heat is evolved on the transition of the hydrogen from the active to the ordinary state, and that this heat merely raises the temperature of the liquid in the pile, without affecting the electromotive force. In the two piles last mentioned, with hydrochloric acid as the liquid, hydrogen separates *in statu nascenti* upon the negative platinum disk; but here, in order to account for the difference in question, we must assume that cold is generated when the hydrogen passes from the above-mentioned state into its ordinary condition—an assumption which contradicts the preceding one. Favre, on this account, ascribes this fact to other, secondary chemical processes which may occur in the pile; but he does not specify in what those processes are to consist. The fact that the galvanic heat which is developed in Smee's pile by the current increases with the inserted resistance, Favre has endeavoured to explain by assuming that the ratio between the primary and secondary processes is, as to its magnitude, dependent on the intensity of the current.

[*] *Ann. de Chim. et de Phys.* [4] t. iv. p. 392 (1865).
[†] Ibid. [3] t. xl. (1854).
[‡] *Comptes Rendus*, t. lxvii. p. 1012 (1868). Compare with this Bosscha's investigation in Pogg. *Ann.* vol. ciii. (1858).
[§] Ibid. t. lxviii p. 1300 (1869).

From the foregoing it follows without doubt that it is very difficult to employ the mode of representation No. 1 for the explanation of the thermal phenomena which take place in the galvanic pile and its circuit. Even if we leave out of consideration that it is by no means easy to understand in what manner the heat is conveyed from the pile to the different conductors outside of it, it may yet be truly said that it has been attempted to attribute the difference between the amounts of the chemical and the galvanic heat to causes whose presence cannot with any certainty be proved, and the actions of which are still less determined quantitatively. It appears to me that such a way of explaining cannot, from a scientific point of view, be called a good one. The question takes another form when representation No. 2 is employed. The assumption that the electromotive force expends a certain quantity of *vis viva* or heat to produce the work of the current is fully justified, because it is valid also for forces different in nature from the electromotive. That the consumption of heat by the electromotive force must be equal to the production of heat by the current is self-evident; yet it does not by any means necessarily follow that this heat-consumption is exactly equal to the quantity of heat which is generated by the chemical processes in the pile.

Employing representation No. 2, the experiments cited in the foregoing show that the heat-consumption occasioned by the electromotive force in the zinc-platinum and cadmium-platinum piles with hydrochloric acid for the electrolytic liquid, is greater than the heat-production brought about by the chemical processes which take place in these piles, but that the ratio in the Smee pile is inverse. It is easy to understand that the consumption of heat, and consequently also the quantity of galvanic heat in the entire circuit, in the Smee pile, must be less when the external resistance is diminished. The negative platinum disk in this pile is polarized by hydrogen; and when currents so feeble as those which occur in these experiments are in question, the polarization increases with the intensity of the current. Therefore, when the external resistance is little, the electromotive force of the polarization must be relatively great, and consequently the total electromotive force of the pile become inconsiderable. It is therefore evident that the heat-consumption of this force, and consequently also the galvanic heat developed by the current, must diminish with the resistance. We have therefore no need to have recourse to unknown causes in order to account for the results obtained by the experiments instituted.

4. For determining indubitably which of the two repre-

sentations, No. 1 or No. 2, deserves to be preferred, we obtain the best clue from the experiments instituted in order to measure the heat-phenomena in a chemical-decomposition cell or voltameter. With this view Raoult employed partly water acidulated with sulphuric acid, and partly a solution of sulphate of copper as electrolytic liquid, and with the help of a mercury calorimeter measured the quantity of heat which arose in the decomposition-cell over and above the quantity occasioned there by the passage of the current in consequence of the resistance [*]. If W is the total heat developed in the voltameter, and gw (as before) the heat which the passage of the current occasions in consequence of the resistance (or the galvanic heat), $W - gw$ was measured. This heat-difference, which Raoult calls the local heat, may in the following be denoted by L. By special experiments Raoult moreover measured the polarization-electromotive force produced in the voltameter during the passage of the current. This electromotive force may be denoted by e, and that of an element of the Daniell pile by d. The series employed consisted, in the different experiments, of from two to twelve Daniell elements. The numbers cited below under L denote the number of heat-units which were developed in the voltameter during the liberation of one equivalent of hydrogen or copper. As, moreover, it cannot be necessary to describe more closely the arrangements in the experiments, we may here add only that, with the decomposition of the sulphuric-acid water, the two poles in the voltameter consisted of platinum wires in the first two experiments (A and B)—while in the third experiment (C) only the positive pole consisted of platinum, the negative being formed of a thick wire of copper. In all three experiments with the copper solution the positive pole consisted of a thick wire of platinum, and the negative of one of copper. As may readily be understood, the voltameter alone was enclosed in the calorimeter, and the piles stood outside of it.

The following were the results obtained :—

On the decomposition of the water.		On the decomposition of the sulphate of copper.	
$\dfrac{e}{d}$.	L.	$\dfrac{e}{d}$.	L.
A. 2·04	+14898	A. 1·59	+7594
B. 1·75	+ 7596	B. 1·58	+7997
C. 2·16	+17626	C. 1·36	+2821

From this we see that in both cases a considerably greater

[*] *Ann. de Chim. et de Phys.* [4] t. iv. p. 411; see also t. ii. p. 317.

quantity of heat is produced than the galvanic which is caused in the voltameter by the passage of the current; and this happens notwithstanding it might be supposed, in consequence of the chemical decomposition of the liquid, that the result would be a cooling.

Raoult is of opinion that the cause of the heating in this case is to be sought in secondary chemical processes occurring in the voltameter, which have nothing to do with the current. He supposes that the constituents of the electrolyte, which cover the electrodes and cause the polarization, are easily decomposable, and on their decomposition give rise to a heat-production in the same manner as takes place with the hyper-oxide of hydrogen—that the decomposition of these products first takes place after they have left the electrodes and begin to ascend through the liquid, in consequence of which this is heated by them without the current being at all affected. In my view, this explanation is unsatisfactory; on the other hand, heat-production is in this case a necessary consequence if we start from representation No. 2.

According to this way of representing it, if a current passes through an electromotor in the direction required by its electromotive force, a quantity of heat is consumed which is proportional to the electromotive force, multiplied by the intensity of the current; but if the current goes in the opposite direction, just as great a quantity of heat is generated. If, therefore, the current is permitted to traverse the electromotor during so long a time that an equivalent of the electrolyte is decomposed, the quantities of heat consumed or generated become proportional to the electromotive force. Consequently there arises in the voltameter a source of heat, because the electromotive force of the polarization acts in the opposite direction against the current which is passing through. It has been mentioned above that the galvanic heat-development in a closed Daniell pile during the liberation of an equivalent of copper amounts to 23900 heat-units; and according to No. 2, exactly so much heat must, during the same time, be consumed by the electromotive force of the pile. With the help of this datum it is easy to calculate the magnitude of the above-mentioned source of heat in the various experiments instituted by Raoult; for we need only multiply $\frac{e}{d}$ with the last-mentioned number. But, moreover, heat is consumed by the chemical decomposition in the voltameter. For each equivalent, 34462 thermal units are consumed, according to Favre and Silbermann, in the decomposition of the water; and, according to Raoult, 29605 in the decomposition of the

salt of copper. If from the quantity of heat developed by the electromotive counterforce the last-mentioned quantity of heat, consumed by the decomposition, be subtracted, we actually obtain, as the following Table shows, the values observed by Raoult of the surplus heat L.

<div align="center">In the decomposition of water.</div>

$$
\begin{array}{llll}
 & & & \text{L.} \\
\text{A } (2{\cdot}04 \times 23900). & 48756 - 34462 & = & +14294 \\
\text{B } \quad,, \quad\quad ,, & 41825 - 34462 & = & +\ 7363 \\
\text{C } \quad,, \quad\quad ,, & 51624 - 34462 & = & +17162 \\
\end{array}
$$

<div align="center">In the decomposition of sulphate of copper.</div>

$$
\begin{array}{llll}
 & & & \text{L.} \\
\text{A } (1{\cdot}59 \times 23900). & 38001 - 29605 & = & +8396 \\
\text{B } \quad,, \quad\quad ,, & 37762 - 29605 & = & +8157 \\
\text{C } \quad,, \quad\quad ,, & 32504 - 29605 & = & +2899 \\
\end{array}
$$

Indeed the calculated do not differ more from the observed numbers than can be accounted for from the unavoidable errors of observation occurring in experiments of this sort. Hence it follows that, to account for the thermal phenomena occurring in the voltameter, there is no need to have recourse to the secondary chemical processes which may take place there, the nature and amount of which are more or less unknown[*]; the theoretical view above presented under No. 2 perfectly suffices for their explanation.

5. Some experiments made by Favre, on the development of heat in the voltameter, fully confirm what is here alleged[†]. In a mercury calorimeter (No. 1) provided with seven muffles, in the first five muffles five equal Smee's elements were enclosed, and in the sixth a rheostat consisting of a platinum wire with so great a resistance that the resistance of the remaining parts of the circuit, in comparison with it, could almost be neglected. On the solution of one equivalent of zinc in each element, the calorimeter indicated the quantity of chemical heat k developed in the five elements.

---

[*] Raoult, in order to show that the occurrence of secondary chemical processes is necessary for the production of the heat in this case, makes an inference which might be rendered in the following manner:—When the voltameter is inserted in the circuit, the electromotive force of the series is thereby diminished by e, and the entire circuit is thus deprived of a quantity of heat which is equal to $23900 \times \dfrac{e}{d}$. This heat is expended in the production of chemical decomposition in the voltameter. But as the quantity of heat expended for this is less than that above-mentioned, the difference must be made good by the occurrence of secondary chemical processes.

[†] *Comptes Rendus,* t. lxvi. p. 252; Pogg. *Ann.* vol. cxxxv. p. 300 (1868).

The same experiment was then repeated, with only this difference, that a voltameter was enclosed in the seventh muffle. In this the quantity of heat $k-a$ was obtained, where a (as shall presently be shown) denotes the quantity of heat which was consumed in the chemical decomposition of the electrolyte in the voltameter. In this way Favre found, as the equivalent for the chemical decomposition of water, 34204, and for the decomposition of sulphate of copper 26568 thermal units.

Thereupon the following experiments were made:—The voltameter was taken out of the calorimeter No. 1, and placed in another calorimeter, No. 2; while the series and rheostat remained in No. 1. It was now found that calorimeter No. 1 indicated, on the electrolysis of water, 54235 heat-units less than in the first experiment—that is, when the series and rheostat were enclosed in No. 1 and no voltameter was inserted in the circuit. When the voltameter contained the copper-solution, 38530 units of heat less than in the first experiment were obtained in the same calorimeter. The calorimeter No. 2 gave, in the former case, a heating of 20335, and, in the latter, one of 12445 units.

Upon this Favre asks, What can be the reason that the large quantity of heat that has disappeared in the series is again found in the calorimeter No. 2? This cause, he continues, cannot be referred to the physical resistance of the voltameter; for this is so insignificant that it might almost be neglected in comparison with the resistance of the rheostat. According to his view the cause is to be sought in the circumstance that the substances which take part in the chemical processes (oxygen, hydrogen, &c.) are found now *in statu nascenti*, now in the ordinary state. This explanation appears to me quite unsatisfactory. Even if it be assumed that such secondary chemical processes take place in the voltameter, they can certainly cause a heating of the voltameter, but they cannot possibly account for the great loss of heat which takes place in the series and rheostat.

[To be continued.]

LIX. *Mode of the Propagation of Sound, and the Physical Condition determining its Velocity on the Basis of the Kinetic Theory of Gases.* *By* S. Tolver Preston [*].

1. SINCE the kinetic theory of gases is now generally accepted by physicists, affording, as it does, a rational explanation of the physical qualities and deportment of gases

in agreement with experiment, it becomes a point of interest
to inquire how the propagation of sound (or the propagation
of waves in gases generally) would be explained by the aid of
the kinetic theory.

Since, in accordance with this theory, the molecules of
gases are in motion among each other in straight lines, colli-
ding among themselves, it would appear somewhat difficult to
form a distinct idea as to the mode of propagation of a wave
in a gas and the condition determining the rate of propagation,
unless some law or guiding principle could be conceived of
according to which the molecules moved. Now I think it
will be found, on considering the subject, that there is a
guiding principle governing the motions of the molecules of
a gas among each other.   I propose to show that the mo-
lecules of a gas in a fixed vessel under the influence of their
mutual collisions tend to arrange their motions in such a
way that an equal number of molecules move at any instant
in any two opposite directions; or a self-acting adjustment
goes on among the molecules of a gas in such a way that
when an imaginary plane is placed in any position outside
the vessel, the number of molecules which at any instant
are approaching the plane is equal to the number which at
the same instant are receding from it.

2. This will be found to be a simple condition following
necessarily from the conditions of equilibrium of pressure of
a gas; for if a preponderating number of molecules were
moving in any special direction in a gas, this would be fol-
lowed by an increased pressure in that direction, whereas ob-
servation shows that this is not the case, or the pressure of a
gas is uniform in all directions.   This therefore proves that
the motion of the molecules which produces this pressure is
uniform in *all* directions (and does not take place in one di-
rection in preference to another), and therefore that the num-
ber of molecules moving in any direction at a given instant is
equal to the number moving in the opposite direction.   It
might be said that some of the molecules moving in one direc-
tion might happen to possess a less velocity than some of those
moving in the opposite direction, and therefore an increased
number of molecules would be required in that direction in
order to produce an equilibrium of pressure ; but it is to be
observed that a space of any perceptible capacity encloses a
vast number of molecules, so that every conceivable velocity
of motion is encountered as much in one direction as in the
opposite direction, and all inequality is thus equalized.   It is
not as if it were a case of a few molecules—say a dozen, when
the mean velocity of the six moving in one direction might

happen to be considerably different from the mean velocity of the six moving in the opposite direction ; but since it is a case of millions of molecules, all inequalities are equalized. It is evident, therefore, that in order that equilibrium of pressure may exist in a gas, or in order that the molecules in their mutual collisions may balance each other's effects, as many molecules must be moving in one direction as in the opposite, so that the *vis viva* in two opposite directions is equal. If, for example, in any portion of a gas, such as a cubic foot, the number of molecules moving towards one imaginary bounding plane of this cubic foot were greater than the number moving towards the opposite bounding plane, the whole cubic foot of gas would tend to be propelled bodily in that direction towards which the greater number of its molecules was moving, thus producing a current, whereas no such currents in gases are observed. Indeed, according to actual observation, each portion of a gas however small, appears to be at rest. The portion of the gas could only be at rest under the condition that it exerted a uniform pressure in *all* directions : for if it exerted a less pressure in any given direction, it would be reacted upon by the surrounding gas and propelled from that side towards which it exerted the least pressure. Since, however, any portion of the gas, however small, appears to be at rest, it follows that this portion of the gas must exert a uniform pressure in all directions, and therefore that the motion of the molecules composing this portion of the gas must take place *uniformly* towards *all* directions, *i. e.* as many molecules must be moving in any one direction as in the opposite.

3. As a direct corollary to this, it may be shown that there is a self-acting tendency for this form of motion to be kept up; or, in other words, a mechanical self-adjustment is continually going on among the molecules of a gas to produce a special character of motion, viz. that the motion of the molecules takes place uniformly towards all directions, or the numbers of molecules moving in any two opposite directions are equal— that, therefore, if by any artificial means the motion of the molecules of a gas could be interfered with or changed, they would, when left to themselves, automatically return back to the above *regular* form of motion.

This evidently follows from the consideration that, since the equilibrium or uniformity of pressure requires that the motion of the molecules should take place uniformly towards all directions, and since any disturbance of this form of motion would disturb the equilibrium of pressure, and since the equilibrium of pressure is self-adjusting, the uniformity of motion

(or the motion of the molecules towards *all* directions) which produces this equilibrium of pressure is therefore also necessarily self-adjusting. This fact will be at once evident by supposing an extreme case. Thus, supposing the component molecules of an imaginary cubic foot of gas to have their motion interfered with in such a way that the molecules only move longitudinally (*i. e.* in directions parallel to each other), then this cubic foot of gas will cease to exert any transverse or lateral pressure; for a pressure cannot be exerted at right angles to the line of motion. The pressure of the surrounding gas will therefore cause this cubic foot of gas to collapse laterally (owing to the absence of a lateral opposing pressure); and in this act of collapse, by the lateral inrush of the surrounding gas, the molecules of this cubic foot will receive a forcible lateral acceleration which they previously wanted, the irregularity of motion being thus soon corrected and the equilibrium of pressure restored. It is clear that in the actual fact no such state of things as this could occur; for the rapid interchange of motion going on among the molecules of a gas necessarily corrects any incipient disturbance of the equilibrium of pressure immediately on its occurrence—a continual self-acting adjustment thus going on which entirely prevents any abnormal movement of the molecules from developing itself, the movement of the molecules equally towards *all* directions being thus *automatically* maintained. To summarize, therefore, we observe :—

That a special form of motion is required to produce equilibrium or uniformity of pressure in all directions within a gas, viz. uniformity of motion in the molecules of the gas towards all directions (so that an equal number of molecules are moving in any two opposite directions); and, further, that this uniformity of motion is self-adjusting, or the gas itself automatically adjusts the motion of its molecules, so that they move uniformly towards all directions.

4. *Mode of the Propagation of Waves.*—These considerations enable the mode of propagation of waves in a gas to be illustrated in a very simple manner. Thus, since the molecules of a gas move in such a way that an equal number of molecules are moving in any two opposite directions, we may therefore represent the molecules of a gas by a row of spheres or ivory balls (fig. 1), colliding among each other in such a way that at any given instant half are moving in one direction and half in the opposite. The odd balls 1, 3, &c. may be supposed to move simultaneously in one direction, during the time that the even balls 2, 4, &c. move

Fig. 1.
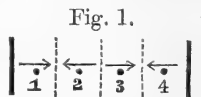
simultaneously in the opposite direction, the balls continually rebounding from each other and maintaining an equilibrium by their collisions. In the annexed diagram I, II, III, IV, V, may serve to illustrate the different phases of the movement. The balls 1, 2, 3, 4 may be supposed to be controlled by the two plane surfaces A and B, from which the end ones rebound, the whole row thus maintaining an equilibrium. Each ball simply performs an oscillatory movement within the limits of space bounded by the dotted lines in the diagram—all the odd balls (or half the row) moving simultaneously in one direction, whilst all the even balls (or the other half of the row) move simultaneously in the reverse direction. To assume the balls to move *simultaneously* merely serves to simplify the conceptions without altering in the least the true conditions of the case. In the actual fact, of course, in the case of a gas, some of the molecules would be moving obliquely to such an imaginary line ; but since the molecules maintain an equilibrium by their collisions, it cannot alter the case in the least if we assume for simplicity the motions to be straight; or, indeed, the resolved component of the motions in the direction of the line can be taken. The row of colliding balls, like the colliding molecules of a gas, thus maintain a perfect equilibrium, the row not tending bodily as a whole to be propelled in any particular direction, but simply tending to open out or expand, and to separate the controlling surfaces A and B. The oscillatory form of motion of the balls fulfils that condition, that the row of balls, as a whole, maintains a fixed position while its parts are in motion, just as a portion of a gas maintains a fixed position while its parts are in motion.

5. To illustrate now the way a wave is propagated by a gas, we may suppose that a forward and backward motion is communicated to the plane A in the form of a vibratory motion ; also the plane B may be supposed removed and the row of spheres extended indefinitely from the plane A, the movement of vibration of the plane being also supposed slow compared with the normal velocity of the spheres. In that case the sphere 1 would strike against the plane A a number of times during one forward swing of the plane. On the commencement of the first forward swing of the plane, the plane advancing towards the sphere 1, the latter receives a small increment of velocity, which it transfers by collision to sphere 2, the two spheres simply exchanging velocities

according to the principles of impact of equal masses. The sphere 1 therefore returns towards the plane with its original normal velocity unchanged, and receives a second similar increment of velocity from the plane, which it again transfers, &c. The sphere 2 at once transfers to sphere 3 the increment of velocity received from sphere 1, the sphere 2 returning with its original normal velocity to repeat the process. The same considerations apply to all the spheres ; and in this way during one forward swing of the plane A, a succession of small increments of velocity are propagated in the form of a wave by exchange of motion along the line of spheres (or the wave consists in a peculiarity in the motion of the spheres such that they move forward with a velocity somewhat greater than the normal velocity, and backward with the normal velocity), the velocity of transmission of the wave being that of the spheres themselves, assuming that the diameter of the spheres is small compared with their mean distance, as is true of the molecules of gases. The length of this pulse or half-wave evidently must depend on the time taken by the plane to make one swing or semivibration ; or wavelength is proportional to vibrating-period. The wave-length will also evidently depend on the normal velocity of the spheres. By the backward swing of the plane, to finish one complete vibration, the plane A moving or receding from the sphere 1, the latter will be slightly retarded ; and thus a succession of small decrements of velocity, forming the second half of the wave, is transmitted in precisely the same manner along the row of spheres: or the second half of the wave consists in a peculiarity in the motion of the spheres such that they move forward with a velocity somewhat less than the normal velocity and backward with the normal velocity.

6. It is of course clear, as before remarked, that in the case of a gas the molecules in their mutual collisions would not all be moving in the direct line of propagation of the wave at the instant of its passage, but some of them more or less obliquely to the line of propagation ; so that, for this cause, the rate of propagation of the wave would be necessarily, to a certain extent, slower than the normal velocity of the molecules themselves. This, however, does not affect in the least the principle involved ; and therefore the above mode of illustration serves to give a perfectly just idea of the physical process by which, through the normal motion of the molecules of a gas, changes of velocity experienced by the molecules or " waves " are propagated to a distance through the gas in accordance with the kinetic theory.

7. *Cause producing the Oscillation of the Mass of Air.*—It is

a known fact that, when a sound-wave passes through a mass of air, the mass of air oscillates, as a whole, backwards and forwards (within small limits). It will therefore be necessary to explain how this takes place in accordance with the kinetic theory. Taking the illustrative case of the row of spheres, we have observed that when *no* wave is passing, each sphere is normally oscillating backwards and forwards within definite limits; or all the alternate spheres, or half the row, move forward, whilst the other half moves backwards. We have observed that when a wave passes, each sphere, after it has transferred the increment of velocity foward, returns backwards with its *normal* velocity; *i. e.* the sphere is only affected with the increment of velocity *when it moves forwards*, and not when it moves backwards. The sphere therefore makes its forward movement with a greater velocity than its backward movement; and accordingly the sphere gains more ground at its forward movement than it loses at the backward movement; and as this occurs at each oscillation of the sphere, and the sphere oscillates a great number of times backwards and forwards during the passage of the wave, there is a gradual gain of ground by the sphere. The same applies to all the spheres forming the first half of the wave, so that all the spheres affected by the increment of velocity are pushed bodily forward during the time that the increment of velocity forming the first half of the wave passes. The same considerations apply to the molecules of a mass of air, which are accordingly pushed bodily forward during the time that the first half of a wave of sound passes; and thus the mass of air oscillates forward as a whole during the passage of the half-wave. This forward movement of the mass of air is naturally accompanied by a condensation of the air.

8. Precisely the same considerations (conversely) apply to the decrement of velocity experienced in the second half of the wave. The spheres now make their forward movement with a decrement of velocity and their backward movement at their normal velocity; so that their forward movement is made at a *less* velocity than their backward movement; and thus (conversely) ground is lost by the spheres, or they are shifted bodily backwards during the time the decrement of velocity forming the second half of the wave passes. The same applies to the molecules of a mass of air, which is accordingly shifted bodily backwards during the passage of the decrement of velocity constituting the second half of the sound-wave; and thus the mass of air oscillates backwards and forwards during the time the complete wave traverses it. By the backward-shifting of the mass of air, a rarefaction ensues.

9. *The Physical Condition determining the Velocity of Sound.*
—It must evidently follow, as a necessary consequence from
the above considerations, that the velocity of sound in a gas
must be proportional to the velocity of the molecules of the
gas ; so that, therefore, the numbers expressing the velocity
of sound in different gases and the numbers expressing the
velocities of the molecules of different gases must be propor-
tional to each other.   This is found to be true.   Thus, for
example, the velocity of the molecules of hydrogen is known
to be four times as great as that of the molecules of oxygen;
so the measured velocity of sound in hydrogen is four times
its measured velocity in oxygen.

10. The velocity of sound in a gas is, as was to be expected
from the reasons before referred to, a certain fixed proportion
slower than the normal velocity of the molecules of the gas.
Thus the velocity of the molecules of hydrogen at 0° C. is
6050 feet per second, whereas the velocity of sound in hydro-
gen at 0° C. is 4164 feet per second.   The constant ratio ex-
pressing the relation between the velocity of sound in a gas
and the velocity of the molecules of the gas is given by the
number 0·688 very nearly; or the velocity of sound in a gas is
0·688 time the velocity of the molecules of the gas ; so that
the velocity of sound in a gas may be simply got by multiply-
ing the known velocity of its molecules by this constant.   It
would seem probable that by taking into account the oblique
motions of the molecules in their collisions along the line of
propagation of the wave at all conceivable angles, by a system
of averages, the absolute value of the velocity of sound in a
gas could be determined independently as an *à priori* problem,
direct from the molecular velocity, by mathematics [*].

11. The fact that the velocity of sound in a gas is simply pro-
portional to the velocity of its molecules cannot surely but be
regarded as a far more simple and satisfactory physical con-
dition governing and determining the velocity of sound than
the vague idea of " elasticity," or (as assumed) that the velo-
city of sound in a gas is proportional to its " elasticity."   The
definite physical conception of the velocity of the molecules
themselves which by their interchange of motion propagate
the wave, is surely far preferable to the vague idea of " elasti-
city " governing the velocity of the wave.

12. *Velocity unaffected by Density or Pressure.*—According
to the principles involved in the kinetic theory, therefore, the
velocity of sound in a gas is dependent on nothing else but
the velocity of its molecules ; or, whatever conceivable condi-
tions the gas may be subjected to (such as change of density

[*] See Postscript (2).

or pressure), the velocity of sound will remain unaltered so long as the velocity of the molecules remains unaltered.

Thus, if the density of a gas (or air) be changed by forcing fresh air into the same space, the velocity of sound will remain unaltered, simply because the velocity of the molecules remains unaltered. The old theory would assume that the velocity of sound has remained unaltered in this case because increased *density* (or increased number of air-molecules) has a power of diminishing the velocity of the sound-wave, while, on the other hand, the increased pressure of the air against the sides of the vessel (considered to represent increased " elasticity ") has a power of increasing the velocity of the wave, and that the two actions counteract each other, and therefore the velocity of the wave has remained unaltered. Contrast this with the simple and realizable explanation of the kinetic theory, viz. that the velocity of the wave has remained unaltered simply because the velocity of the molecules which propagate it has remained unaltered.

13. Clausius has demonstrated that, for a gas to fulfil Mariotte and Gay-Lussac's laws :—

(1) " The space actually filled by the molecules of the gas must be infinitesimal in comparison with the whole space occupied by the gas itself."

(2) " That those portions of the path of a molecule throughout which the molecular forces are of influence in sensibly altering the motion of the molecule either in direction or velocity must be of vanishing value compared with those portions of the path throughout which such forces may be considered as inactive."

Since, therefore, the portion of a molecule's path through which it is acted on by other molecules of the gas is vanishingly small compared with the range of its path throughout which it is not so acted on, there is therefore practically no distance action between the molecules of a gas, which accordingly can only influence each other by direct impact. The only way, therefore, one molecule of a gas can influence another is by moving up to it and striking against it. The only way, therefore, a wave or small impulse can be propagated from molecule to molecule through a gas is by the molecule possessing the impulse moving up to and striking against another molecule ; and therefore the velocity of propagation of such wave or impulse must depend solely and entirely upon the velocity with which the molecule moves ; or the sole conceivable cause regulating the velocity of an impulse propagated from molecule to molecule is the *velocity of the molecule itself*, or the velocity with which the molecule traverses its free path.

This is an inevitable certainty by the acceptance of the kinetic theory; and therefore it is clear that a change of density of the air, by adding to the number of air-molecules (as when air has been forced into a vessel), cannot possibly influence the velocity of propagation of a wave by the air so long as the molecular velocity remains constant.

14. Obviously when air has been forced into a vessel and thereby its density increased, this has simply the effect, by adding to the number of molecules, of increasing the number of collisions among the molecules; but this cannot affect in the least the velocity of propagation of the sound-wave, for the simple reason that it cannot affect the velocity of the molecules. So the increased pressure of the air against the sides of the vessel (considered to represent increased " elasticity ") cannot possibly influence the velocity of the sound-wave, this increased pressure being merely due to an increased number of molecules colliding against the sides of the vessel.

The above considerations may perhaps be made more obvious by imagining the case of a number of couriers propagating a message with no intervening mechanism through which they can communicate with each other (as in analogy with the molecules of a gas). Then the velocity of propagation of the message will solely depend on the velocity of the couriers themselves. Increased number of couriers (corresponding to increased number of molecules in a gas or increased density) will have no effect on the velocity of propagation of the message, provided the velocity of each courier remains the same. So with the molecules of a gas, which according to the kinetic theory, are interchanging motion among themselves with no means of acting upon each other excepting by direct impact. Thus the velocity of a wave in a gas can be determined solely by the velocity of its molecules, and by nothing else; *or the sole physical condition determining the velocity of sound in a gas is the velocity of its molecules.*

15. *Variation of Specific Gravity.*—So, therefore, also variation of specific gravity in gases can have no influence on the velocity of the sound-wave, unless the molecular velocity be changed. One known consequence of the kinetic theory is, that equal volumes of different gases all contain the same number of molecules, so that therefore the specific gravity of a gas is proportional to its molecular weight. A cubic foot of oxygen contains the same number of molecules as a cubic foot of hydrogen; but the specific gravity of oxygen is sixteen times that of hydrogen, and the oxygen molecule is sixteen times as heavy as the hydrogen molecule, and (as is known)

the molecules of hydrogen are moving four times as fast as the molecules of oxygen; and on this account the wave is propagated four times as fast, not because the propagating molecule is heavy or light.

16. *Case of Heated Rarefied Air.*—When air unconfined and free to expand is heated, the velocity of sound is found to be increased. This, according to the old theory, is considered to be due to the diminished *density* of the air, or the diminished number of molecules attendant on the expansion of the air. Now, in accordance with the kinetic theory, this increased velocity of propagation of the wave is simply explained by the increased *velocity* of the air-molecules which propagate it, attendant on the application of heat to the air, the motion of the molecules representing the "heat" and their velocity being a measure of the "temperature." The increase in the velocity of the molecules of air attendant on the application of heat pushes the surrounding air back, and causes the heated portion to be rarefied, thus diminishing its density. The density of the heated portion may therefore be taken as a convenient measure of the velocity of its component molecules, upon which the velocity of propagation of the sound-wave depends. Thus it may be a convenient rule that the velocity of sound in heated rarefied air is inversely proportional to the square root of the density of the air, the cause of this being that the molecular velocity is itself inversely proportional to the square root of the density (*i. e.* in air free to expand and change its density). The diminished density of the heated rarefied air cannot, however, be said to be the *cause* of the increased velocity of the sound-wave; the diminished density is rather the *effect* of the increased molecular velocity, which itself is the *cause* of the increased velocity of the wave.

17. So the velocity of sound in different gases is found to be inversely proportional to the square root of the specific gravities of the gases : but this is simply due to the fact that the specific gravity of a gas is as its molecular weight; and the molecular velocity, upon which the velocity of propagation of the sound-wave depends, is itself inversely proportional to the square root of the molecular weight.

18. *Case of Heated Confined Air.*—When air (or any gas) is *confined* in a vessel so as to prevent expansion and then heated, the velocity of sound is found to be augmented. This augmentation of velocity, according to the kinetic theory, is due to the *same* cause as in the previous case (when the air was unconfined), viz. to the increased *velocity* of the air molecules attendant on the application of heat. According to the old theory, the augmented rate of propagation of the wave is

now referred to increased " elasticity " of the air (represented by the increased *pressure* of the confined air upon the sides of the vessel). This increased pressure upon the sides of the vessel, however, is only due to the increased *velocity* of the air-molecules which impinge against the sides of the vessel, and whose velocity has been augmented by the application of heat (and whose motion represents the " heat "). The increased pressure of the heated air may serve as a convenient means for estimating the augmentation of molecular velocity, upon which the augmented velocity of propagation of the sound-wave depends. Thus it may be a convenient rule that the velocity of sound in heated (confined) air (of unchanged density) is proportional to the square root of the " elasticity " of the air (as measured by the *pressure*), the reason of this being that the velocity of the molecules of air (which determines the velocity of the propagation of the wave) is itself proportional to the square root of the pressure. The augmented pressure of heated confined air is evidently not itself the *cause* of the increased velocity of propagation of the wave ; but the augmented pressure (" elasticity ") is the *effect* attendant on the increased velocity of the molecules of air, which increased velocity is itself the *cause* of the augmented velocity of propagation of the wave.

19. The result of these considerations may therefore be summarized as follows :—

That the velocity of propagation of a wave (such as a wave of sound) in a gas is solely determined by, and proportional to, the velocity of the molecules of the gas ; that this velocity of propagation of the wave is not affected by density, pressure, or by the specific gravity of a gas, or by any thing else excepting the velocity of its molecules.

20. This, it may be observed, is a condition following inevitably on the acceptance of the kinetic theory ; and surely the very simplicity of this relation as affording a definite physical conception of the condition determining the velocity of sound, and as giving an insight into its mode of propagation, would be by itself sufficient to recommend it over the old system. If any thing I have written should serve to divert the attention of others more competent than myself to this interesting subject, the purpose of this paper will have been served.

PS. (1). It is proper for me to add that this paper is not wholly original, but the perusal of a paper by Mr. J. J. Waterston in the ' Philosophical Magazine ' (Jan. 1859, Sup. to vol. xvi.) formed the starting-point of the present paper. The method of illustrating the propagation of a wave by means of a line of spheres is due to Mr. Waterston ; but the

investigation as to how the special motion assigned to the line of spheres can properly represent the character of the motion of the molecules of a gas in its normal state is my own, no special investigation of this kind being contained in Mr. Waterston's paper. To Mr. Waterston, however, is mainly due the initiative in this subject. As I should be sorry to claim any originality that I did not possess, I would respectfully direct the attention of readers to the portion of Mr. Waterston's paper bearing on this subject.

PS. (2). Professor Clerk Maxwell, to whom this paper was communicated, and who has taken a kindly interest in the subject, has worked out mathematically the velocity for a wave or impulse propagated by a system of particles moving among each other according to the conditions of equilibrium investigated in the first part of this paper—the diameter of the particles being assumed so small as to be negligible compared with their mean distance, and the particles being further assumed spherical, so that there is no movement of rotation developed at the encounters (which would involve loss of velocity).

Under these premises, the velocity of the wave was found to be $\frac{\sqrt{5}}{3}$ (or 0·745) into the mean velocity of the particles.

In most gases the velocity of sound is slightly less than this. This is referable to the movements of rotation developed at the encounters of the molecules (which calculably would delay the wave to a certain extent). In vapour of mercury, according to the determinations of Kundt and Warburg, the velocity of sound is exactly $\frac{\sqrt{5}}{3}$ into the molecular velocity.

London, May 1877.

LX. *Crystallographic Notes. By* W. J. LEWIS, *M.A., Fellow of Oriel College, Oxford, and Assistant in the Mineral Department, British Museum*[*].

[Plate IV.]

Barium Nitrate.

LAST autumn my friend Mr. T. Davies, of the British Museum, kindly brought me a fairly large crystal with a very large number of faces on it. It had been found at the

bottom of a reagent-bottle which had been put aside for many years. The solution, owing to a faulty stopper, had all eva-porated, leaving this single crystal. The label had been lost; so, after a crystallographic investigation, I scraped off a very small portion from a part on which were no crystal-faces. By means of this I was able to determine that the crystal was one of barium nitrate. I have thought that a description of its crystallography would be interesting, both on account of the remarkable development of its faces and its decided tetar-tohedrism. The facts already known about barium nitrate are that it crystallizes in the cubic system, shows a hemi-hedrism with parallel faces, and has the forms $\{1\,0\,0\}$, $\{1\,1\,1\}$, and $\pi\{2\,1\,0\}$.

The forms observed on the crystal in question are $a = \{1\,0\,0\}$, $t = \kappa\pi\{1\,2\,4\}$, $h = \kappa\pi\{\bar{1}\,2\,4\}$, $n = \kappa\pi\{5\,3\,1\}$, $l = \kappa\{3\,1\,1\}$, $s = \kappa\{\bar{2}\,1\,1\}$, $o = \kappa\{\bar{1}\,1\,1\}$. A glance at the stereographic pro-jection (fig. 1, Plate IV.) of these forms shows that t, n, and l occupy alternate octants, and that o, s, and h occupy the remaining octants. The forms t and h are complementary, and make up the hemihedral form with parallel faces $\pi\{1\,2\,4\}$. The physical character of the faces of this form in adjacent octants manifests, however, the tetartohedrism of the crystal; for the faces h are large and smooth, the faces t small and rough. The faces n are tetartohedral and well developed in alternate octants; they are for the most part bright; but the most care-ful examination in the remaining octants failed to discover the slightest trace of corresponding planes. Similarly the faces o, which are large and bright, were found only in alternate oc-tants. The faces l are fairly developed and bright. The faces s are very small but fairly bright. The faces of the cube a are large and bright.

The principal zones on the crystal are those containing the planes $a\,n\,l_{\prime}$, $a_{\prime\prime}\,s_{\prime\prime}\,o$, $a\,t_{\prime}\,t\,h\,s_{\prime\prime}\,h_{\prime}$, $o\,l_{\prime\prime}\,n_{\prime\prime}\,h_{\prime}$. These afforded considerable aid in seeking for traces of planes, and also in some instances in determining the real positions of some of the badly developed faces. The following are the most im-portant angles of the combination, with which the measure-ments accorded well:—

$$
\begin{array}{lll@{\qquad}lll@{\qquad}lll}
a\,t_{\prime} & 29° & 12' & \bar{a}\,n & 32° & 19' & \lceil o\,l_{\prime\prime} & 58° & 31' \\
a\,t & 77 & 24 & a\,n_{\prime} & 80 & 16 & \mid o\,n_{\prime\prime} & 72 & 58\tfrac{1}{2} \\
a\,t_{\prime\prime} & 64 & 7\tfrac{1}{2} & a\,n_{\prime\prime} & 59 & 32 & \lfloor o\,h_{\prime} & 112 & 12\tfrac{1}{3}
\end{array}
$$

The stereographic projection (fig. 1) shows very clearly the zones and the arrangement of the poles. Fig. 2 is an ortho-gonal projection on one of the faces of the cube; and fig. 3 is

one in which the axes have been projected in the usual manner. To avoid confusion, the small planes *s* have been omitted in the latter figure. The crystal is elongated in a direction nearly coinciding with the normal to a face of the tetrahedron. An oval ring seems to have been first deposited; and on this the crystal has grown, vaulting itself on the lower surface so as not to cling to the bottle. There were no definite crystal-faces to be seen on this concave portion. The top is also irregular and indefinite. The crystal introduced into the polarizing microscope between crossed Nicols depolarized the light. Exact experiments on the rotation of the plane of polarization could not be made without destroying the crystal.

On some crystals of barium nitrate crystallized out of solution during the course of a few weeks, the forms $\kappa\{1\,1\,1\}$, $\kappa\{\bar{1}\,1\,1\}$, $\{1\,0\,1\}$, and $\kappa\{\bar{1}\,2\,2\}$ were found. The planes $\kappa\{\bar{1}\,1\,1\}$ seemed to be smoother and brighter than those of $\kappa\{111\}$; and the edges of the former carried the planes $\kappa\{\bar{1}\,2\,2\}$.

Sphene.

On a crystal from the Tyrol, obtained by me some years ago, two rough ill-developed planes are situated on the quoin formed by the two planes $n=\{1\,2\,3\}$ and the base $c=\{0\,0\,1\}$. They look almost like the result which would be produced by slightly grinding down this quoin. The exact symmetry of the two planes, as also the frequency of their occurrence, show them, however, to be really planes. Hessenberg, who devoted considerable attention to this mineral, has noticed similar faces on the crystals from the Zillerthal, described in his *Min. Notizen*, vi., and has introduced them in two of the figures of these crystals. As this part of the *Notizen* is out of print, I have copied one of these figures (fig. 34), in which the small triangular dotted planes are those under consideration. Hessenberg says that he found them more or less clearly developed on almost all the crystals from this locality. He expresses, however, his conviction that the rough portion is only a continuation of the plane *n*.

Lately I obtained several crystals on which these planes were very fairly developed, of one of which fig. 4 is a projection. They give such excessively bad reflections, that it was only by observing with a ray of sunlight thrown into the room by a mirror, and by slightly oiling the surfaces, that reliable measurements were obtained. The form calculated from these measurements is $\{3, 3, 10\}$, adopting the axial system given in Miller's 'Mineralogy.' The following are the angles observed and calculated:—

		Observed.	Calculated.
$n\,\phi$	....	$11°\ \ 2'$	$10°\ \ 59'$
$n_{,}\,\phi$	....	$33\ \ 33\frac{1}{4}$	$33\ \ 19$
$\phi\,\phi_{,}$	....	$22\ \ 33\frac{1}{2}$	$22\ \ 31$
$c\,\phi$	....	$27\ \ 42\frac{1}{2}$	$27\ \ 47\frac{1}{3}$

Gold.

Measurement of a large though imperfect crystal in the British Museum showed it to be a combination of the cube with the tetrakishexahedron {4 1 0} and the triakisoctahedron {8 1 1}. As the faces were very dull, and but rough measurements could be obtained, I was glad to confirm this observation by the examination of a crystal showing the same combination in Mr. Ludlam's beautiful collection, which he was good enough to lend me. The angles measured on the latter crystal agree fairly well with the calculated angles.

		Measured.	Calculated.
(4 1 0, 8 1 1)	....	$9°\ \ 25'$	$9°\ \ 52'$
(4 1 0, 4 0 1)	....	$20\ \ 18$	$19\ \ 44$

I saw recently a very beautiful crystal of the same combination in the collection of the École des Mines, Paris. In the two former the planes {8 1 1} are deeply striated parallel to their intersection with the faces of the cube.

LXI. *Acoustical Observations.*
By Lord Rayleigh, *M.A., F.R.S.*[*]

Perception of the Direction of a Source of Sound.

IN a paper with the above title, communicated last year to the Musical Association and afterwards published in abstract in 'Nature,' I brought forward the fact that we are unable to distinguish whether a pure tone (obtained from a tuning-fork and air-resonator) is immediately in front of or immediately behind us—although with other sounds, and notably with the human voice, there is in general no difficulty. In order to make the experiment satisfactorily, it is necessary to provide two similar forks and resonators and to place the observer between them. At a given signal *both* forks are struck, but one of them only is held over its resonator. If this precaution be neglected, the noise attending the excitation of the fork vitiates the experiment. Subsequently to the reading of my paper, it occurred to me that if the ordinary

view as to the functions of the two ears be correct, there must be other ambiguous cases besides those already experimented upon. To the right of the observer, and probably nearly in the line of the ears, there must be one direction in which the ratio of the intensity of sound as heard by the right ear to the intensity as heard by the left ear has a maximum value greater than unity. For sounds coming from directions in front of this the ratio of intensities has a less and less value, approaching unity as its limit, when the sound is immediately in front. In like manner, for directions intermediate between the direction of maximum ratio and that immediately behind the observer, the ratio of intensities varies continuously between the same maximum value and unity. Accordingly, for every direction in front there must be a corresponding direction behind for which the ratio of intensities has the same value; and these two directions could not be distinguished in the case of a pure tone. The only directions as to which there would be no ambiguity are the direction of maximum ratio itself, and a corresponding direction of minimum ratio on the other side of the head.

The attitude of my mind with respect to this result was, I confess, one of considerable scepticism. A great number of miscellaneous experiments had been made with forks as well as with other sources of sound; and I thought that, if these ambiguities had existed, indications of them must have been perceived already. It was therefore with some curiosity that I took the first opportunity, last September, of submitting the matter to the test of experiment, the same forks (making 256 vibrations per second) being used as on previous occasions. The decision was soon given. An observer facing north, for example, made mistakes between forks bearing approximately north-east and south-east, though he could distinguish without a moment's hesitation forks bearing east and west. In all such experiments it is necessary that the observer keep his head perfectly still, a very slight motion being sufficient in many cases to give the information that was previously wanting.

A suggestion was made, in the discussion that followed the reading of my paper before the Musical Association, which I thought it proper to examine, though I had not much doubt as to the result. In order to meet the difficulty in the ordinary view as to the functions of the two ears arising out of the fact that a 256-fork seems to be heard nearly as well with the ear turned away as with the ear turned towards it, it was suggested that possibly the discrimination between forks right and left depended on something connected with the commence-

ment of the sound. It might be supposed, for example, that we are able to recognize which ear is first affected. On trial, however, it appeared that the power of discrimination was not weakened, although the observer stopped his ears during the establishment of the sound.

When *one* ear is stopped, mistakes are made between forks right and left; but the direction of other sounds, such as those produced by clapping hands or by the voice, is often told much better than might have been expected.

The Head as an Obstacle to Sound.

The perfection of the shadow thrown by the head depends on the pitch of the sound. I have already mentioned that it appears to make but little difference in the audibility of a pure tone with a frequency of 256, whether the ear used be turned towards or from the source. But the case is very different with sounds of higher pitch, such as that of an ordinary whistle. The one that I employed was blown from a loaded gas-bag, and gave a very steady note of pitch f^{IV}. A hiss is also heard very badly with the averted ear. This observation may be made by first listening with both ears to a steady hiss on the right or left, and then closing one ear. It makes but little difference when the further ear is closed, but a great difference when the nearer ear is closed. A similar observation may be made on the sound of running water.

For the same reason a hiss or whisper, coming from a person whose face is averted, is badly heard. Under these circumstances even ordinary speech is difficult to understand, though the mere intensity of sound does not seem deficient.

Reflection of Sound.

In many cases sound-shadows appear much less perfect than theory would lead us to expect. The anomaly is due in great measure, I believe, to an error of judgment, depending on the enormous range of intensity with which the ear is capable of dealing. The whistle of a locomotive is very loud at a distance of ten yards. At a mile off the intensity must be 30,000 times less; but the sound still appears rather loud, and would probably be audible under favourable circumstances even when enfeebled in the ratio of a million to one. For this reason it is not easy to obtain complete shadows; but another difficulty arises from the fact that there are generally obstacles capable of reflecting a more or less feeble sound into what might otherwise be a nearly complete shadow. An attempt to examine this point led me to a few simple experiments on the reflection of sound, which may be worth recording.

The principal obstacle throwing the shadow was the corner of a large house ; and among the sources of sound tried were the human voice, tuning-forks, whistles steadily blown, and a small electric bell, of which the last (which was employed in Professor Reynolds's acoustical experiments) proved to be as convenient as any. The source was placed close to the south side of the house, at a distance of eight or ten yards from the south-west corner, while the observer took up a corresponding position on the west side. With these arrangements the sound-shadow was pretty good, though far from perfect. When, however, a flat reflector, such as a drawing-board of moderate dimensions, was held at the proper angle by an assistant placed at some distance outwards from the corner, the augmentation of sound was immense, and the hearer realized for the first time how very good the shadow really was.

A screen made by stretching a ' Times ' over a hoop about $2\frac{1}{2}$ feet in diameter gave apparently as good a reflection as the drawing-board ; but when calico was substituted for the paper the reflecting-power was very feeble. By wetting the calico, however, it could be made to reflect very well. These results are in agreement with the striking experiments described by Professor Tyndall.

Audibility of Consonants.

I suppose it must have been noticed before now that the *s* sound is badly returned by an echo. Standing at a distance of about 150 yards from a large wall, I found that there was scarcely any response to even the most powerful hiss. *Sh* was heard a little better ; *m, k, p, g* pretty well ; *r* very well ; *h* badly ; *t* badly ; *b* seemed half converted into *p* by the echo. The failure of the hiss seems to be the fault of the air rather than of the wall, for a powerful hiss heard directly at a distance of 200 yards had very little *s* left in it.

Interference of Sounds from two unisonant Tuning-forks.

In ordinary experiments on interference the sounds are only approximately in unison, and consequently the silences resulting from antagonism of the vibrations are of only momentary duration. I thought it of interest, therefore, to arrange an experiment in which the sounds should be pure tones, absolutely in unison, and should proceed from sources at a considerable distance apart. With the aid of electromagnetism the solution of the problem was comparatively easy. An intermittent electric current, obtained from a fork interrupter making 128 vibrations per second, excited by means of electromagnets two other forks, whose frequency was 256. These latter

forks were placed at a distance of about ten yards apart, and were provided with suitably tuned resonators by which their sounds were reinforced. The pitch of both forks is necessarily identical, since the vibrations are *forced* by electromagnetic forces of absolutely the same period. The arrangement was successful; and with a battery-power of two Grove cells sounds of fair intensity were obtained. With one ear closed it was possible to define the places of silence with considerable accuracy, a motion of about an inch being sufficient to produce a marked revival of sound. At a point of silence, from which the line joining the forks subtended an angle of about 60°, the apparent striking up of one fork, when the other was stopped, had a very peculiar effect.

Symmetrical Bell.

I do not know whether it has ever been noticed that there ought to be no sound emitted along the axis of a symmetrical bell. It is easy to see that at any point of the axis any effect, whether condensation or rarefaction, which may be produced by one part of the surface of the bell must be neutralized by other parts, and that therefore on the whole there can be no variation of pressure during the vibration. The experiment may be made with a large glass bell (such as those used with air-pumps), set into vibration by friction with the wetted finger carried round the circumference. If the axis of the vibrating bell be turned exactly towards the observer, the sound is feeble as compared with that heard when the position of the bell is altered. The residual sound may be due to want of symmetry, or more probably to reflexion from the ground, which last cause of error it is almost impossible to get rid of.

Octave from Tuning-forks.

When a vibrating fork is held over an air-resonator in tune with itself, the sound emitted is very approximately a pure tone ; but when the fork is placed in contact with a sounding-board, the octave may generally be perceived by a practised ear, and is often of remarkable loudness. By means of a resonator tuned to the octave the fact may be made apparent to any one. This result need not surprise us. By the construction of a fork the moving parts are carefully balanced, and the motion is approximately isolated. In the ideal tuning-fork, composed of equal masses moving to and fro in a straight line, the isolation would be complete, and there would be no tendency whatever to communicate motion to surrounding bodies. In an actual fork, however, even if the direction of motion of the masses were as nearly as possible perpendicular to the

stalk, the necessary curvature of the paths would give rise to an unbalanced centrifugal force tending to set the sounding-board in vibration. The force thus arising is indeed of the second order, and might probably be neglected, were it not that the apparatus is especially suited to bring it into prominence.

In order to test the soundness of this view as to the origin of the octave, the following experiment was contrived. A 256 tuning-fork was screwed on to a resonance-box intended for a 512 tuning-fork, and therefore approximately in tune with the octave of the first fork. When a powerful vibration was excited by means of a bow, *the octave sound was predominant*, and but little could be heard of the proper tone of the fork. In order to place the two sounds on a more equal footing, a resonator, consisting of a bottle tuned by pouring water into it to a frequency of 256, was brought near the ends of the vibrating prongs. By adjusting the distance it was easy to arrange matters so that at the beginning of the vibration neither sound had a conspicuous advantage. But, as the amplitude of vibration diminished, the graver tone continually gained on its rival, and was left at last in complete possession of the field. The purity of the remaining sound could be tested at any time by the perfection of the silence obtained by removing the air-resonator. This arrangement may be recommended to any one who wishes to practise his ears in hearing octaves.

From the above experiment (in which, if desired, the ear may be replaced by König's manometric flames), it appears that the octave sound is to be attributed to a motion of the second order, which is rendered important by the peculiar isolation of the motion of the first order. The harmonic sounds heard when suitably tuned resonators are presented to the free ends of the prongs, though also dependent on orders of the motion higher than the first, have a somewhat different origin.

Influence of a Flange on the Correction for the Open End of a Pipe.

In theoretical investigations[*] as to the amount of the correction to the length of an open pipe due to the inertia of the external air, it has been usual, for the sake of facilitating the calculations, to suppose that the open end is provided with an infinite flange. Even with this simplification no exact solution of the problem has been obtained. It has been proved, however, that, provided the wave-length be sufficient in relation

[*] Helmholtz, *Crelle,* 1860. Also a memoir by myself " On Resonance," Phil. Trans. 1871.

to the diameter of the pipe, the addition which must be supposed to be made to the length is very nearly equal to, though somewhat less than, ·8242 R, and is certainly greater than ·785 R*, R being the radius of the pipe.

It is obvious that the removal of the flange would make a considerable difference, probably reducing the correction below the lower limit above mentioned. In the absence of any theoretical estimate, I thought it desirable to make an experimental determination of the effect of a flange, and ordered some years ago a pair of similar organ-pipes of circular section for the purpose. My idea was to tune the pipes to unison, and then to count the beats when the pitch of one of them was slightly lowered by the addition of a flange ; but the experiment lay in abeyance until last winter. Instead of tuning the pipes to unison, I preferred simply to count the beats before and after the addition of the flange, which consisted of a large sheet of stiff millboard perforated with a hole sufficiently large to allow the passage of the pipe. In this way it appeared that the effect of the flange was to reduce the frequency by nearly $1\frac{1}{2}$ out of about 242. If we take the velocity of sound at 1123 feet per second, corresponding to 60° F., the calculated effective length of the pipe is about 28 inches, and the radius is 1 inch. Thus the correction to the length due to the flange is the same fraction of 28 inches that $1\frac{1}{2}$ is of 242, or is equal to about ·2R. Combining this result with the theoretical estimate above referred to, we may conclude that the whole correction for an open end, when there is no flange, must be about ·6 R.

Mr. Bosanquet, to whom I communicated the result at which I had arrived, informs me that he has since determined the correction for a flange as ·25 R.

The Pitch of Organ-pipes.

The whole correction to the length of an organ-pipe, necessary to make it agree with Bernoulli's theory, is considerably greater than any of those spoken of under the preceding heading. According to the rule of Cavaillé-Coll, the addition for an open pipe of circular section amounts to as much as $3\frac{1}{3}$ R, whereas for a simple tube open at both ends it should be only about 1·2 R. This discrepancy is, I believe, often attributed to a peculiar action of the stream of air by which the pipe is excited. Of course it is not to be denied that some disturbance arises from this source, as is proved by the dependence of the pitch on the strength of the wind ; but the near agreement between

* See note to a paper "On the approximate Solution of certain Problems relating to the Potential," Math. Soc. Proc. vol. vii. No. 93.

theory and measurements by Wertheim and others, on the pitch
of resonators caused to speak by a stream of air, has always
seemed to me to prove that a comparatively small part only of
the whole discrepancy is to be explained in this way.   On the
other hand, it is obvious that the " open " end at the base of
the pipe is very much contracted, and that the correction
thence arising may be several times as great as that applicable
to the upper end, where the pipe retains its full section.   I was
therefore anxious to ascertain what was the proper note of an
organ-pipe, regarded as a freely vibrating column of air, and
thus to estimate in what proportion the two causes of disturb-
ance contribute to the final result.

   There are two methods by which the pitch of a resonator
may be determined without the use of a stream of air.   The
simplest, and in many cases the most accurate, method con-
sists merely in tapping the resonator with the finger or other
hammer of suitable hardness, and estimating with the aid of a
monochord the pitch of the sound so produced.   In attempting,
however, thus to determine the pitch of the organ-pipe, I
found a difficulty arising from the uncertain character of the
sound, and the results were by no means so accordant as I
desired.   Possibly an observer gifted with a more accurate
ear than mine would have been more successful.   The other
method is one of which I have had a good deal of experience,
and which I can generally rely upon to give results of mode-
rate accuracy.   It consists in putting the ear into communi-
cation with the interior of the resonator, and determining to
what note of the scale the resonance is loudest.   I have gene-
rally found it possible thus to fix the pitch of a resonator to
within a quarter of a semitone.   In the present case a small
hole was made in the side of the pipe near the centre ; and over
the hole a short piece of tube was cemented, which could be
put into communication with the ear by means of a rubber
tube.   In this way the effective length of the pipe was deter-
mined to be 28·7 inches, 4·7 inches more than the actual
length.   As a check upon this estimate, I closed the upper
end of the pipe with a plate of wood and again determined the
note of maximum resonance.   The effective length of the pipe
was now 29·1 inches, so that the correction due to want of
openness at the lower end amounted to 5·1 inches.   If we add
·6 as a correction for the upper end, we obtain as the corrected
length of the pipe in its ordinary condition 29·7 inches.   The
difference between this and 28·7, obtained directly, is greater,
I think, than can be ascribed to errors of experimenting, and
is possibly connected with the excessive magnitude of the cor-
rection in relation to the wave-length of the sound.   The

actual note of the pipe, when blown in the ordinary way by a wind of pressure measured by $2\frac{1}{2}$ inches of water, corresponded to an effective length of 28 inches, so that the blown note was actually *higher* in pitch than the note of maximum resonance. So far, therefore, from the depression of pitch in an organ-pipe below that calculated from the actual length, according to Bernoulli's theory, being principally due to the action of the wind, it would appear that in the absence of a peculiar action of the wind the depression would be even greater than it is. Too much stress, however, must not be laid on a single observation; and all I would maintain is, that by far the larger part of the depression of pitch is due to the insufficient openness of the lower end of the pipe.

May 9, 1877.

LXII. *Notices respecting New Books.*

The Elements of Machine Design: an Introduction to the Principles which determine the Arrangement and Proportion of the Parts of Machines, and a Collection of Rules for Machine Design. By W. Cawthorne Unwin, *B. Sc., Assoc. Inst. Civil Engineers, Professor of Hydraulic and Mechanical Engineering at the Royal Indian Civil Engineering College.* London: Longmans, Green, and Co., 1877. [Text-Books of Science, small 8vo. Pp. 326.]

THE author takes as a motto for his work an extract from Reuleaux's 'Theoretical Kinematics,' of which the following is part:—"Machine design has been removed by Redtenbacher from its incorrect position as a part of Applied Mechanics, and established on a footing of its own. Its province is to show how the parts of the machine are to be proportioned so as to resist deformation. In order to accomplish this fully, they must be considered both with reference to the external forces acting on the machine, and the corresponding molecular forces within its substance." These words serve as a general description of the author's aim; and accordingly he begins the volume with an account of the theory of the strength of materials, with a view to its application to the questions which form the subject of the volume. These may be briefly enumerated as fastenings, pipes, shafts, bearings, gearing, linkwork, and valves. It is almost needless to say that, though to ensure a due degree of strength is an important element in designing machinery, many other points are brought under notice; and accordingly the work is not so exclusively occupied with the question of strength as the motto might lead the reader to expect. Thus, in the chapter on toothed gearing, our author first considers the cases of shafts driven by rolling contact, and proceeds to show the need of teeth for transmitting force by this means. After briefly mentioning the materials employed, he defines the parts and proportions of teeth, investigates the conditions which deter-

mine their form, and gives rules for describing cycloidal and invo-
lute teeth. He then takes the question of the strength of teeth,
goes on to give rules for the construction and proportion of wheels,
and ends the chapter with some articles on screw-gearing. This
very brief account of the contents of a single chapter will perhaps
serve to indicate the sort of treatment which the subject receives
at our author's hands, as well as to show how much is taken up
directly with the question of strength, viz. about a fourth part in
the case of the present chapter. A similar remark would apply to
other chapters.

The work seems to have been executed with great care and with
an ample knowledge of the subject. It will doubtless be very useful
to students of mechanical engineering; and those whose interest
in mechanics is of a less practical kind will find a good many inter-
esting questions worked out clearly and accurately.

The author of such a work as that before us has one great diffi-
culty to contend with. He is obliged to consider the extent of the
mathematical knowledge which his readers may be presumed to
have; and in order to render his work useful to as large a class
as possible, he is obliged to take some things for granted which
admit of being proved. What should be taken for granted and
what proved is a question that must be settled by a sort of com-
promise; and as the author may be presumed to have given it a
great deal of attention, his opinion, as expressed in the selection
that he makes, is *primâ facie* entitled to great weight; we do not,
however, think that in the present case Mr. Unwin has been
always very happy. At all events, we think that we were entitled
to expect that the omissions should be obviously consistent with
each other, that where matters are not referred to their ultimate
principles they should, at all events, be referred to important pro-
positions (to what may be called secondary principles), and that
when algebraical formulæ are given without proof they should be
accompanied with sufficient explanations to ensure their being
understood. It would be easy to point out examples of failure in
each of these respects, which might with advantage be rectified in
a second edition. Thus, on p. 186 it is taken for granted, with-
out so much as a reference, that the work lost in the friction of
teeth is proportional to $p \, \mathrm{H} \, (\mathrm{R}_1 + \mathrm{R}_2) \div \mathrm{R}_1 \, \mathrm{R}_2$; while on p. 207–8
the formula for the tensions of a rope stretched over a fixed
cylinder ($\mathrm{T}_1 = \mathrm{T}_2 \, e^{\mu\theta}$) is proved at full length. It is hard to see
why both should not be assumed, or both proved. In the former
case the reader's attention should be drawn to the fact that a
point capable of proof is being taken for granted; this, however,
is not done on p. 186. Again, on p. 30, where the subject dis-
cussed is the strength of a beam subject to simple bending, we are
told that the beam will be of adequate strength when the bending-
moment equals $f \mathrm{Z}$, Z being "the modulus of the section—that is, a
function of the dimensions of the section which is proportional to
the moment of resistance of the section;" and on p. 35 a table is
given of the values of Z for certain forms of section. Here every

thing is arbitrary unless the reader knows more than he finds in the book, whereas, if the author had gone back a single step, and stated the connexion between Z and the moment of inertia of the cross section, the reader would have seen that nothing more was being assumed than certain geometrical results which are to be found proved in many text-books[*]. On p. 54, and elsewhere, Poncelet's formulæ for approximating to $\sqrt{(a^2 + b^2)}$ are used, but not a word is said to indicate the degree of approximation obtained by them. When a student is informed that if a and b are unknown he may write $0\cdot83\,(a + b)$ for $\sqrt{(a^2 + b^2)}$, he certainly ought to be told that in doing so he is liable to an error of rather more than a sixth part of the whole, either in excess or defect. A somewhat glaring example of a formula given without the needful explanations occurs on p. 31, where, in an article on Continuous Beams, the author gives the equation which expresses the " Theorem of Three Moments :" he states in connexion with the equation that the bending-moments at the extreme points of support are zero. Now, if the reader happens to know more about the subject than the author tells him, he will be able to understand the article, and will see that as far as it goes it is quite correct. If, however, he attempts to use the equation with no more information than the book supplies, we should feel no certainty as to the result : *e. g.*, let the beam be supported on the two extreme points and an intervening point (A, C, and B), and let the reader suppose that B is gradually brought nearer and nearer to C ; when it reaches C he will probably expect that as B is now at the extreme point the bending-moment at it will be zero. The equation, however, tells him that it equals one fourth part of the moment of the weight of the beam. The explanation of this seeming paradox would hardly occur to him, viz. that when the beam is said to *rest* on three supports this means that when B and C are less than a certain distance apart the end of the beam must be pressed down on C, and that when they come into coincidence the force by which this is done must be infinitely great. Again, when the author states, almost parenthetically, that the points of support are at the same level, the unlearned reader is not likely to understand that the sameness of the level is an essential condition of the truth of the formula, and that a very small difference between the heights of the points of support would render the equation quite inapplicable. It would be easy to name cases in which a difference of no more than an exceeding small fraction of an inch would completely alter the conditions of the question.

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[*] On a subsequent page (p. 50) the relation is stated, but not in such a way as to invalidate what is said above.

LXIII. *Proceedings of Learned Societies.*

[Continued from p. 392.]

Nov. 16, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

THE following paper was read:—

"Experimental Contributions to the Theory of the Radiometer."
—Preliminary Notice.   By William Crookes, F.R.S. &c.

Instead of bringing another preliminary notice before the Society,
I should have preferred reserving the announcement of my new
results on the Repulsion resulting from Radiation until they were
fit to be offered in a more complete form; but the radiometer is
now so much occupying the attention of scientific men, and results
of experiments with this and allied instruments are appearing so
frequently in the scientific journals at home and abroad, that were
I not to adopt this method of bringing the results of my more re-
cent experiments before men of science, I might find myself
anticipated in some or all of the conclusions at which I have
arrived.

On June 15th last I mentioned to the Society that the re-
pulsion resulting from radiation increases up to a certain point
as I exhaust the air from the torsion-apparatus.   After long-
continued exhaustion the force of radiation approaches a maximum,
and then begins to fall off.   I have since succeeded in experi-
menting at still higher exhaustions, and with different gases in
the apparatus; and by means of a McLeod gauge attached to the
mercury pump I have been able to measure the atmospheric
pressure at any desired stage of exhaustion.   I have not only
measured the force of repulsion, but also the viscosity of the
residual gas; and from the results I have plotted the observa-
tions in curves, which accompany this paper, and which show how
the viscosity of the residual gas is related to the force of repulsion
exerted by radiation.   These curves must not, however, be con-
sidered as representing more than the broad facts; for I have not
included in them my final observations, which in all probability
will introduce modifications in them.

In plotting these curves I have supposed my scale to be 1000
metres long, and to represent one atmosphere.   Halfway up the
scale therefore, or 500 metres, represents half an atmosphere; 999
metres up the scale represents an exhaustion of $\frac{1}{1000}$ of an atmo-
sphere: each millimetre, therefore, stands for the millionth of an
atmosphere.

My results have principally been obtained at the top of the scale;
and it is the last quarter of a metre which supplies the diagrams ac-
companying this paper.

When the residual gas is air, the viscosity (measured by the loga-
rithmic decrement of the arc of oscillation) is practically con-
stant up to an exhaustion of 250 millionths of an atmosphere, or
0·19 millim. of mercury, having only diminished from 0·126 at the

normal pressure of the atmosphere to 0·112. It now begins to fall off: at 200 millionths it is 0·110, at 100 millionths it is 0·096, at 50 millionths it is 0·078, at 20 millionths it is 0·052, at 10 millionths it is 0·035; and at 0·1 of a millionth of an atmosphere
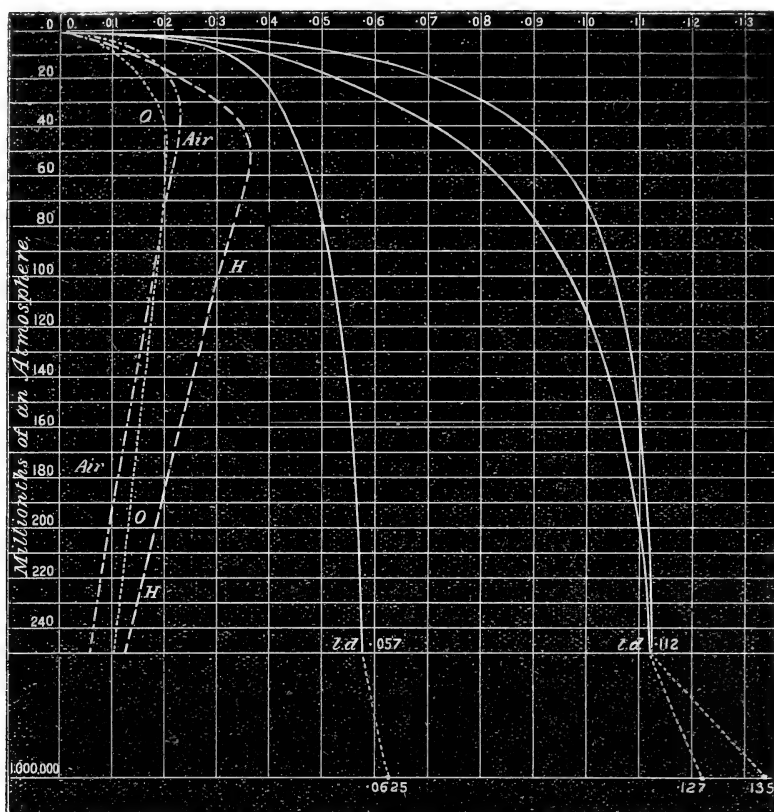
the log. dec. has fallen to about 0·01. Simultaneously with this decrease in the viscosity, the force of repulsion exerted on a black surface by a standard light varies. It increases very slowly till the exhaustion has risen to about 70 millionths of an atmosphere; at about 40 millionths the force is at its maximum; and it then sinks very rapidly, till at 0·1 millionth of an atmosphere it is less than one tenth of its maximum. On continuing the curves of the log. dec. and the force of radiation, and assuming that the torsion-fibre of glass has no viscosity, it is most probable that they both would come to zero when the last traces of an atmosphere had been taken out of the apparatus.

The oxygen diagram differs from that of air. The log. dec. is 0·126 at the atmospheric pressure; it falls to 0·111 at a pressure

of 250 millionths of an atmosphere; at 100 millionths it is 0·105, at 50 millionths it is 0·093, at 20 millionths it is 0·068, and at 2 millionths it is 0·02. The force of repulsion in oxygen increases very steadily up to an exhaustion of about 40 millionths of an atmosphere; it is at its maximum at about 30 millionths, and thence declines very rapidly.

Hydrogen gives a remarkable diagram. The viscosity at the normal pressure is measured by a log. dec. of 0·063; at 250 millionths of an atmosphere it is 0·057, at 100 millionths it is 0·052, at 50 millionths it is 0·046, whence it rapidly sinks. The force of repulsion increases slowly up to an exhaustion of 250 millionths, then quickly until it attains its maximum at about 50 millionths, and it then rapidly declines. The force of repulsion is very great in a hydrogen vacuum, being in comparison with the maximum in an air vacuum as 70 to 41. Neither is it necessary to get so high an exhaustion with hydrogen as with other gases to obtain considerable repulsion. This shows that in the construction of radiometers it is advantageous to fill them with hydrogen before exhausting.

Carbonic acid has a viscosity of about ·01 at the normal pressure, being between air and hydrogen, but nearer the former. On approaching a vacuum, the force of repulsion does not rise very high, and soon falls off.

Before working with this apparatus I thought that monohydrated sulphuric acid evolved no vapour, and I therefore freely used it for cleaning out the pump and for drying the gases. I can even now detect no vapour-tension; but a comparison of the curves, with and without sulphuric acid, shows that the presence of this body modifies the results. One of my curves represents the action of the residual sulphuric anhydride gas. The experience thus gained has led me to adopt phosphoric anhydride for drying the gases. I can detect no ill effects from the presence of this agent; and I have been able in consequence to push the rarefaction to higher points than before.

The McLeod gauge will not show the presence of mercury vapour. It is therefore possible that I have a greater pressure in the apparatus than is here stated. I have, however, entirely failed to detect the presence of mercury vapour at any great distance from the mercury in the pump; and the tube packed with gold-leaf, which I frequently interpose between the pump and the apparatus, shows no trace of bleaching, and exerts no appreciable effect one way or the other on the results.

With this pump, assisted sometimes by chemical absorption, it is not difficult to exhaust a radiometer to such a point that it will not move to a candle placed a few inches off; but I have not yet succeeded in stopping the movement of the beam in the torsion-apparatus.

A long series of observations have been taken, at different degrees of exhaustion, on the conductivity of the residual gas to the spark from an induction-coil. Working with air, I find that at a pressure

of about 40 millionths of an atmosphere, when the repulsive force is near its maximum, a spark, whose striking-distance at the normal pressure is half an inch, will illuminate a tube having aluminium terminals 3 millimetres apart. When I push the exhaustion further, the $\frac{1}{2}$-inch spark ceases to pass; but a 1-inch spark will still illuminate the tube. As I get nearer to a vacuum more power is required to drive the spark through the tube; but at the highest exhaustions I can still get indications of conductivity when an induction-coil actuated with five Grove's cells, and capable of giving a 6-inch spark, is used.

When so powerful a spark is employed there is great danger of perforating the glass, thus causing a very slight leakage of air into the apparatus. The log. dec. now slowly rises, the repulsive force of the candle increases to its maximum, and then slowly diminishes to zero, the log. dec. continuing to rise till it shows that the internal and external pressures are identical. With a fine perforation several days are occupied in going through these phases, and they take place with such slowness and regularity as to afford opportunities for getting valuable observations.

The improvements now added by Mr. Gimingham to the pump render it so easy to obtain high exhaustions, that, in preparing experimental radiometers, I prefer to exhaust direct to one or two millionths of an atmosphere. By keeping the apparatus during this exhaustion in a hot-air bath heated to about 300° C. for some hours, the occluded gases are driven off from the interior surface of the glass and the fly of the radiometer. The whole is then allowed to cool, and attenuated air from the air-trap is put in in small quantities at a time, until the McLeod gauge shows that the best exhaustion for sensitiveness is reached; if necessary, this point is also ascertained by testing with a candle. Working in this way, I can now do in a few hours what formerly required as many days. In this manner, employing hydrogen instead of air for the gaseous residue, and using roasted mica vanes set at an angle with the axis, as described further on, I can get very considerably increased sensitiveness in radiometers. I am still unable, however, to get them to move in moonlight. The statements made by an observer nearly a year ago, that he obtained strong rotation by moonlight, must therefore be considered erroneous. My most sensitive torsion-balance will, however, move easily to moonlight.

The above-mentioned facts, in addition to what has already been published, leave no reasonable doubt that the presence of residual gas* is the cause of the movement of the radiometer. But few theories are sufficiently strong not to require reinforcement; and in the present case very much remains to be ascertained as regards

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* It is a question whether the residual gas in the apparatus, when so highly attenuated as to have lost the greater part of its viscosity, and to be capable of acquiring molecular movement palpable enough to overcome the inertia of a plate of metal, should not be considered to have got beyond the gaseous state, and to have assumed a fourth state of matter, in which its properties are as far removed from those of a gas as this is from a liquid.

the mode of action of the residual gas. The explanation, as given by Mr. Johnstone Stoney, appears to me the most probable; and having stood almost every experimental test to which I have submitted it, I may assume for the present that it expresses the truth. According to this the repulsion is due to the internal movements of the molecules of the residual gas. When the mean length of path between successive collisions of the molecules is small compared with the dimensions of the vessel, the molecules rebounding from the heated surface, and therefore moving with an extra velocity, help to keep back the more slowly moving molecules which are advancing towards the heated surface; it thus happens that though the individual kicks against the heated surface are increased in strength in consequence of the heating, yet the number of molecules struck is diminished in the same proportion, so that there is equilibrium on the two sides of the disk, even though the temperatures of the faces are unequal. But when the exhaustion is carried to so high a point that the molecules are sufficiently few and the mean length of path between their successive collisions is comparable with the dimensions of the vessel, the swiftly moving, rebounding molecules spend their force, in part or in whole, on the sides of the vessel; and the onward crowding, more slowly moving molecules are not kept back as before, so that the number which strike the warmer face approaches to, and in the limit equals, the number which strike the back, cooler face; and as the individual impacts are stronger on the warmer than on the cooler face, pressure is produced, causing the warmer face to retreat.

I have tried many experiments with the view of putting this theory to a decisive test. The repulsive force being due to a reaction between the fly and the glass case of a radiometer, it follows that, other things being equal, the fly should revolve faster in a small bulb than in a large one. This cannot well be tested with two different radiometers, as the weight of the fly and the amount of friction would not be the same in each; but I have constructed a double radiometer which shows this fact in a very satisfactory manner. It consists of two bulbs, one large and the other small, blown together so as to have a wide passage between them. In the centre of each bulb is a cup, held in its place by a glass rod; and in the bulbs is a small four-armed fly with roasted mica disks blacked on one side. The fly can be balanced on either cup. In the smaller bulb there is about a quarter of an inch between the vanes and the glass, whilst in the larger cup there is a space of half an inch. The mean of several experiments shows that in the small bulb the fly rotates about 50 per cent. faster than in the large bulb, when exposed to the same source of light.

One of the arms of another radiometer was furnished with roasted mica disks blacked on alternate sides. The other arm was furnished with clear mica disks. The two arms were pivoted independently of each other; and one of them was furnished with a minute fragment of iron, so that by means of a magnet I could

bring the arms into contact, the black surface of the mica then having a clear plate of mica in front of it. On bringing a lighted candle near the instrument, and allowing it to shine through the clear plate on the blackened mica, the clear plate is at once driven away till the arm sets at right angles to the other.

Two currents of force, acting in opposite directions, can exist in the same bulb. I have prepared a double radiometer in which two flies are pivoted one over the other, and having their blackened sides turned in opposite directions. On bringing a lighted candle near, the flies rapidly rotate in opposite directions.

Experiment shows that the force can be reflected from a plane surface in such a manner as to change its direction. If an ordinary radiometer is exposed to light the black surface is repelled, owing to the excess of pressure acting between it and the glass. If, however, a plate of mica were to arrest this force and reflect it back again, the motion should be reversed. Experiment shows that this is the case. A two-disk radiometer was made, having flat opaque mica disks blacked on one side. In front of the black surface of the mica, about a millimetre off, is fixed a large disk of thin clear mica. On bringing a candle near, the molecular pressure streaming from the black surface is caught by the clear plate and thrown back again, causing pressure behind instead of in front; and the result is rapid rotation in the negative direction, the black side now moving towards the light.

To still further test this view of the action, I made another radiometer, similar to the above, but having a clear mica disk on each side of the ordinary mica vane. This prevents the reflection of the pressure backwards, and causes it to expend itself in a vertical plane, the result being an almost total loss of sensitiveness.

The above actions can be explained on the "evaporation and condensation" theory, as well as by that of molecular movement; and I therefore devised the following test to decide between these two theories. A radiometer has its four disks cut out of very clear and thin plates of mica, and these are mounted in a somewhat large bulb. At the side of the bulb, in a vertical plane, a plate of mica, blacked on one side, is fastened in such a position that each clear vane in rotating shall pass it, leaving a space between of about a millimetre. If a candle is brought near, and by means of a shade the light is allowed to fall only on the clear vanes, no motion is produced ; but if the light shines on the black plate, the fly instantly rotates as if a wind were issuing from this surface, and keeps on moving as long as the light is near. This could not happen on the evaporation and condensation theory, as this requires that the light should shine intermittently on the black surface in order to keep up continuous movement.

By cutting a thin plate of aluminium into the form of a spiral, then drawing it out corkscrew fashion, blacking the upper surface and suspending it on a point, a spiral radiometer is made, which rotates like a screw on exposure to light. Here also the black surface need never be in darkness, the pressure acting continuously

between the black side of the spiral and the cylindrical tube in which it is mounted.

The experiments with the double radiometer of different sizes showed that the nearer the absorbing surface was to the glass, the greater was the pressure produced. To test this point in a more accurate manner, a torsion-balance was fitted up with a glass suspending-fibre and reflecting-mirror, as described in my previous papers. At one end of the beam is a disk of roasted mica blacked on one side. In front of this black surface, and parallel to it, is a plate of clear mica, so arranged that its distance from the black surface can be altered as desired, at any degree of exhaustion, without interfering with the vacuum. This apparatus is very sensitive, and gives good quantitative results. It has proved that when light falls on the black surface molecular pressure is set up, whatever be the degree of exhaustion. At the atmospheric pressure this disturbance can only be detected when the mica screen is brought close to the black surface, and it is inappreciable when the screen is moved away. As the barometer-gauge rises, the thickness of the layer of disturbance increases. Thus, retaining the standard candle always the same distance off, when the gauge is at 660 millims., the molecular pressure is represented by 1 when the space separating the screen from the black surface is 3 millims., by 3 when the intervening space is reduced to 2 millims., and by 5 when the space is 1 millim. With the gauge 722 millims. high, the values of the molecular pressure for the spaces of 3, 2, and 1 millim. are respectively 3, 7, and 12. When the gauge is at 740 millims., the corresponding values for spaces of 3, 2, and 1 millim. are 11, 16, and 23. With the gauge at 745 millims., the molecular pressures are represented by 30, 34, and 40, for spaces 3, 2, and 1 millim. When the gauge and barometer are level, the action is so strong that the candle has to be moved double the distance off, and the pressures when the intervening spaces are 12, 6, and 3 millims. are respectively 60, 86, and 107. A large series of observations have been taken with this apparatus, with the result not only of supplying important data for future consideration, but of clearing up many anomalies which were noticed, and of correcting many errors into which I was led at earlier stages of this research. Among the latter may be mentioned the speculations in which I indulged as to the pressure of sunlight on the earth.

Hitherto most of my experiments had been carried on with bad conductors of heat. To get the maximum action of a radiometer it appeared necessary that no heat should pass through to the back surface, but that all should be kept as much as possible on the surface on which the light fell*. At first I used pith; but since

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* I have already shown that when a ray of *light* from any part of the spectrum falls on a black surface the ray is absorbed and degraded in refrangibility, warming the black surface and being emitted as radiant heat. In this sense only can the repulsion resulting from radiation be called an effect of heat.

learning the advantage of raising the whole apparatus to a high temperature during exhaustion, I have used roasted mica lampblacked on one side for the vanes; for this purpose it is almost perfect, being a good absorber on one face, a good reflector on the other, a bad conductor for heat, extremely light, and able to stand high temperatures. Many experiments have been tried with metal radiometers, some of the results being recorded in previous papers which I have read before the Society; but being less sensitive than pith or mica instruments, I had not hitherto worked much with them. I now tried similar experiments to the above, using the best conductors of heat instead of the worst; and for this purpose thick gold-leaf was selected for the surface on which to try the action of radiation. An apparatus was constructed resembling a radiometer with an opening at the top, capable of being closed with a plate of glass. Through this I could introduce disks of any substance I liked, mounted in pairs on an aluminium arm rotating on a needlepoint. The first disks were of gold-leaf, blacked on alternate sides. After exhaustion, a candle repelled the black surface of one of the disks, but, to my surprise, it strongly attracted the black surface of the other disk. I noticed that the disk which moved the negative way was somewhat crumpled, and had the outer edge curved so as to present a slightly concave black surface to the candle. I soon found that the curvature of the disk was the cause of the anomaly observed; and experiments were then tried with disks of gold and aluminium—the latter being chiefly used as being lighter and stiffer, whilst it acted in other respects as gold.

A radiometer the fly of which is made of perfectly flat aluminium plates, lampblacked on one side, is much less sensitive to light than one of mica or pith; but, as I proved in my earlier papers, it is more sensitive to dark heat. Exposed to light, the black face of a metal radiometer moves away as if it were black pith. When, however, it is exposed to dark heat, either by grasping the bulb with the warm hand, dipping it into hot water, or covering it with a hot glass shade, it rapidly rotates in a negative direction, the black advancing, and continuing to do so until the temperature has become uniform throughout. On now removing the source of heat, the fly commences to revolve with rapidity the positive way, the black this time retreating as it would if light shone on it. Pith or mica radiometers act differently from this, dark heat causing them to revolve in the same direction as light does.

The outer corners of the aluminium plates, which were mounted diamond-wise, were now turned up at an angle of 45°, the lampblacked surface being concave and the bright convex. On being exposed to a candle, scarcely any movement was produced; when one vane was shaded off the other was repelled slightly, but the turned-up corner seemed to have almost entirely neutralized the action of the black surface. A greater amount of the same corner was now turned up, the fold going through the centres of adjacent sides. Decided rotation was now produced by a candle, but the black

surface was *attracted* * instead of repelled.   Dark heat still caused the opposite rotation to light, repelling the black surface.

The plates were now folded across the vertical diagonal, the black surface being still inside and the bright metal outside.   The actions with a candle and hot glass shade were similar to the last, but more decided.

The plates were now flattened, and put on the arms at an angle, still being in the vertical plane.   When the bright surface was outside, scarcely any action was produced by a candle; but when the lampblacked surface was outside, strong repulsion of the black was produced, both with a candle and with a hot shade.

The square aluminium plates were mounted in the experimental apparatus, one being attached to the arm by the centre of one of the sides, and the other by an angle.   The opposite corner of the one mounted diamond-wise was turned up at an angle.   The outer convex surface of the diamond plate was blacked, and the side of the square plate facing the same way was also blacked, so that either two black or two bright surfaces were always exposed to the light, instead of a black and a white surface, as is usual in radiometers.   As might have been expected, both these black surfaces were repelled; but the turned-up corner of the diamond-mounted plate proved so powerful an auxiliary to its black surface, that strong rotation was kept up, the square plate being dragged round against the action of light.

Folding the plates with the angle horizontal has not so decided an action as when the fold is vertical.

Sloping the plates and disks of a lampblacked mica radiometer so as to have the black outside, and consequently more facing the side of the bulb, greatly increases its sensitiveness.

The above experiments show that shape has even a stronger influence than colour.   A convex bright surface is strongly repelled, whilst a concave black surface is not only not repelled by radiation but is actually attracted.

I have also tried carefully shaped cups of gold, aluminium, and other metals, as well as cones of the same materials.   I will briefly describe the behaviour of a few typical radiometers made with metal cups, which I have the honour of exhibiting to the Society.

No. 1035.   A two-disk cup-shaped radiometer, facing opposite ways; both sides bright.   The disks are 14·5 millims. in diameter; and their radius of curvature is 14 millims.

Exposed to a standard candle 3·5 inches off, the fly rotates continuously at the rate of one revolution in 3·37 seconds.   A screen placed in front of the concave side so as to let the light shine only on the convex surface, repels the latter, causing continuous rotation at the rate of one revolution in 7·5 seconds. When the convex side is screened off so as to let the light shine only on the concave side, continuous rotation is produced at the

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* I use the word attraction in these cases for convenience of expression. I have no doubt that what looks like attraction in these and other cases is really due to *vis a tergo.*

rate of one revolution in 6·95 seconds, the concave side being attracted.

These experiments show that the repulsive action of radiation on the convex side is about equal to the attractive action of radiation on the concave side, and that the double speed with which the fly moves when no screen is interposed is the sum of the attractive and repulsive actions.

No. 1037.　A two-disk cup-shaped aluminium radiometer, as above, lampblacked on the concave surfaces.

In this instrument the action of light is reversed—rotation taking place, the bright convex side being repelled, and the black concave attracted.

That this attraction is not apparent only, is proved by shading off the sides one after the other.　When the light shines only on the bright convex side no movement is produced; but when it shines on the black concave side, this is attracted, producing rotation.

No. 1038.　A cup-shaped radiometer similar to the above, but having the convex surfaces black and the concave bright.

Light shining on this instrument causes it to rotate rapidly, the convex black surface being repelled.　No movement is produced on letting the light shine on the bright concave surface; but good rotation is produced when only the black convex surface is illuminated.

No. 1039.　A cup-shaped radiometer like the above, but blacked on both sides.

With this a candle causes rapid rotation, the convex side being repelled.　On shading off the light from the concave side the rotation continues, but much more slowly; on shading off the convex side the concave is strongly attracted, causing rotation.

When either of these four radiometers is heated by a hot shade or plunged into hot water, rotation is always produced in the opposite direction to that caused by the light.　On removing the source of heat the motion rapidly stops, and then commences in the opposite direction (*i. e.* as it would rotate under the influence of light), the rotation continuing as long as the fly is cooling.　Chilling one of these radiometers with ether has the opposite action to exposing it to dark heat.

The vanes of radiometers have also been formed of metal cones, and of cups and cones of plain mica, roasted mica, pith, paper, &c.; and they have been used either plain or blacked on one or both surfaces.　These have also been balanced against each other, and against metal plates, cups, and cones.　The results are of considerable interest, but too complicated to explain without great expenditure of time and numerous diagrams.　The broad facts are contained in the above selections from my experiments.

The action of light on the cup-shaped vanes of a radiometer probably requires more experimental investigation before it can be properly understood.　Some of the phenomena may be explained on the assumption that the molecular pressure acts chiefly in a direction normal to the surface of the vanes.　A convex surface would therefore cause greater pressure to be exerted between itself and the

bounding surface of glass than would a concave surface. In this way the behaviour of the cup-shaped radiometer with both surfaces bright, No. 1035, can be understood, and perhaps also that of Nos. 1038 and 1039. It would not be difficult to test this view experimentally, by placing a small mica screen in the focus of a concave cup, where the molecular force should be concentrated. But it is not easy to see how such an hypothesis can explain the behaviour of No. 1037, where the action of the bright convex surface more than overcomes the superior absorptive and radiating power of the concave black surface ; and the explanation entirely fails to account for the powerful attraction which a lighted candle is seen to exert on the concave surfaces in Nos. 1035, 1037, and 1039.

LXIV. *Intelligence and Miscellaneous Articles.*

RESEARCHES ON THE METALLIC REFLECTION OF POLARIZED OBSCURE HEAT-RAYS. BY M. MOUTON.

I HAVE employed, in these researches, one of the apparatus ordinarily used by M. Desains for studying heat-spectra. Polarized in a determinate azimuth, the light and heat traversed first a plate of flint glass suitably inclined to the plane of incidence, and intended to nnul the effects produced by the prism *. The pencil, reflected, then dispersed and analyzed, was finally resolved into a very pure spectrum, the luminous portion of which was directed to the slit of the thermoelectric pile. The breadth of this slit was 1 millim. ; that of the red band of the luminous spectrum 4 millims., and the total extent of the luminous spectrum about 4 centims.

I operated on three wave-lengths (λ_1, λ_2, λ_3) distributed in the obscure portion of the spectrum, and sensibly symmetric in reference to the extreme red :—λ_1, of the yellow ; λ_2, of the blue-green ; and λ_3, of the indigo. The method of experiment rests on principles established by M. Jamin in his " Études de la réflexion métallique de la lumière"†:—

1. Every ray initially polarized in any other azimuth than zero and 90° becomes, after reflection, elliptic.

2. After passing through a spar prism, of which only the extraordinary image is utilized, an elliptic ray presents, when the principal section of the prism coincides with the major axis of the ellipse, a maximum of intensity—with the minor axis, a minimum ; and if we study these intensities in pairs of azimuths of α and $\alpha + 90°$, going from the major to the minor axis, the first predominates over the second as long as α is comprised between the major axis and 45°, becoming inferior to it as soon as α passes beyond that bisectrix of the axes. Thus the source of heat is only required to be constant during each pair of observations, a period which is rendered very brief by a special movement permitting the analyzer to be rotated rapidly through 90°. The azimuth of the bisectrices of of the axes of the ellipse can thus be determined within 1 degree.

* Fizeau and Foucault, *Ann. de Chim. et de Phys.* 3e série, t. xxx. p. 147.
† Ibid. t. xix. p. 321 *seqq.*

3. The azimuth of the incident vibration can thus be brought to be such that the two principal components of the reflected vibration will be equal. This azimuth is again determined by pairs of observations whose only inferiority to the preceding is their not having a constant sum (the source being so), but which, like them, have the advantage of being swift, grouped in pairs independent of one another, and terminated by two equal values of the galvanometric deflections.

4. If we designate by ω the azimuth of the bisectrices of the axes of the ellipse (the incident ray vibrating at 45°), by a the azimuth of the incident vibration which renders the two principal reflected components equal, by δ the difference of phase produced by the reflection, and, lastly, by I and J the absolute numbers by which the reflection multiplies the amplitude of the principal components, we have the two relations

$$\cos \delta = \frac{\cot 2\omega}{\tan 2a} \text{ and } \frac{\text{I}}{\text{J}} = \tan a \quad \text{to determine } \frac{\text{I}}{\text{J}} \text{ and } \delta *.$$

If the Tables (p. 479) be compared with that given by M. Jamin† for the red, the analogy is remarkable. In each of them the rate is seen to increase with the incidence, pass through $\frac{\lambda}{4}$ at 76° with the red, at 79° with λ_1, at 82° with λ_2, and at 83°·5 with λ_3. To these incidences always corresponds a minimum value of the amplitude-ratio.

Let us now turn our attention to the two quantities which " enter as constants into the formulæ of metallic reflection, viz. (1) the incidence of the polarization restored after two reflections from parallel mirrors, (2) the polarization-azimuth of the reflected ray under this incidence when the initial azimuth is equal to 45° "‡. The first is the incidence for which the difference of rate is a quarter of an undulation; it is therefore 79° for λ_1, 82° for λ_2, 83°·5 for λ_3. As to the second, if we designate it by β, we have $\tan \beta = \frac{\text{I}^2}{\text{J}^2}$, whence $\tan \beta = \tan^2 a$; we then find for β the values 15° 40′ for λ_1, 14°·5 for λ_2, and 13° 20′ for λ_3. And, on comparing these results with the Table§ in which M. Jamin has given the values of these two principal quantities, proceeding from the violet to the red, we see that the azimuths of the restored polarization, which diminish for steel from the violet to the red (from 21° to 16° 20′), continue to do so beyond, while the principal incidences, which increase from the violet to the red (from 73° to 77° 52′), continue the series of their increasing values in proportion as we advance into the obscure radiations.

Permit me, in conclusion, to express my acknowledgements to M. Desains: his assistance and daily counsels have been a great help to me in carrying out this investigation.—*Comptes Rendus de l'Académie des Sciences*, April 2, 1877, tome lxxxiv. pp. 650–653.

* See M. Jamin's "Discussion théorique," *Ann. de Chim. et de Phys.* 3ᵉ série, t. xix. p. 276.   † Ibid. t. xix. p. 317.
‡ Ibid. t. xxii. p. 313.   § Ibid. p. 316.

Extract from a Table of Experiments.

(Wave-length λ_2; incidence 80°; mirror of steel; incident vibration 45°.)

Investigation of ω.

Azimuths of the analyzer.	Galvanometer-deflections in millims. to 80 centims.	Azimuths of the analyzer.	Galvanometer deflections.
70°	150	58°	125
70+90	100	58+90	126
60	130		
60+90	120	13	65
56	120	13+90	185
56+90	128		

Value at the axes.

Investigation of a.

Incident vibration.	Analyzer 0°.	90°.
30°	10·5	110°
29	102	100

Therefore $\omega = 58°$, $a = 29°$.

Results of Experiments made on polished Steel.

Wave-length λ_1.

Incidence.	Difference of rate as a function of λ_1.	Ratio of amplitudes. Angle a.	$\dfrac{1}{J}$.
70°	0·179	35°	0·70
75	0·200	33	0·65
79	0·253	28	0·53
80	0·260	29	0·55
82	0·320	30	0·58

Wave-length λ_2.

Incidence.	Difference of rate.	Ratio of amplitudes. a.	$\dfrac{1}{J}$.
70°	0·130	32·5°	0·64
75	0·155	32	0·62
80	0·202	29	0·55
81	0·224	27	0·51
82	0·247	27	0·51
83	0·296	29	0·55

Wave-length λ_3.

Incidence.	Difference of rate.	Ratio of amplitudes. a.	$\dfrac{1}{J}$.
75°	0·120	31°	0·60
80	0·207	29	0·55
81	0·212	27·5	0·52
82	0·225	27	0·51
83·5	0·250	26	0·49

ON THE DIFFUSION OF VAPOURS THROUGH CLAY CELLS.
BY DR. J. PULUJ.

The apparatus for the experiments in this investigation con-
sisted essentially of a clay cell enclosed in a tin-plate box, and
connected with a cooling-apparatus and a vertical glass tube. A
moderate current of air passed through the box, while the vapour
flowed into the cell, and, striking past at the walls of it, diffused
reciprocally with the air in the box. The surplus vapour, as well
as the air that had diffused through, passed into the cooling-
apparatus, where the former condensed, and the air, saturated with
vapour at the temperature of the room, flowed into the glass tube.
The volume of this air was measured by means of soap-films or
very thin disks of mica suspended by soap-water in the tube, by
which the pressure could be preserved equal on the two sides of
the cell. The outflowing air from the box passed through an ab-
sorption-apparatus, the increase in weight of which consequently
gave the quantity of vapour that had diffused through in a fixed
time ; and from this the volume of the vapour was calculated.
Two series of experiments, carried out with steam between 123°·8–
145°·3 and 136°·6–144°·9 C., gave the result, that, *while the ratio
of the volumes of the transdiffused air and vapour remains constant
and is almost exactly equal to the square root of the reciprocal value
of the vapour-density, the velocity of the diffusion increases with the
temperature.* Experiments with the vapours of alcohol and ether
gave for this ratio somewhat higher values than the numbers cal-
culated from the theoretical vapour-densities. The deviation which
had been already observed from Graham's law is not of equal
amount in the case of every vapour ; and the author makes it
appear not improbable that the forces acting between the molecules
of substances and their vapours, which with some vapours are
even more striking, may modify the square-root ratio, and that a
case would not be inconceivable in which a vapour of greater
density would diffuse through a porous plate more rapidly than
one of less density, as is the case with absorbent films of liquid—a
reversal of the diffusion-ratio which has also been observed in the
osmosis of liquids.

*Meanwhile it is to be regarded as certain that the vapours investi-
gated diffuse through clay cells in nearly the inverse ratio of the
square root of their densities.*

In the appendix to his memoir, the author discusses Dufour's
experiments on the diffusion of dry and moist air through porous
plates, demonstrates the inadmissibility of Dufour's assumption
that *dry air diffuses more rapidly than moist* of which the density
is less than that of the former, remarks that certain experiments
made by Dufour himself must be left unexplained by that assump-
tion ; and, starting from the presupposition of the result obtained
from the experiments he has described, *that aqueous vapour diffuses
more quickly than air*, gives a complete explanation of the experi-
ments of Dufour.—*Sitzungsb. der k. Akademie in Wien, math.-
naturw. Classe*, 1877, No. vii. pp. 69–71.

LXV. *The Influence of Light upon the Electrical Resistance of Metals. By* Dr. R. BÖRNSTEIN, *Assistant in the Physical Institute, Heidelberg University* [*].

THE object of the experiments described in this paper is to prove the more general occurrence of a class of phenomena which had hitherto been noticed only within narrow limits, but which, nevertheless, were of such a nature as to excite considerable interest among physicists in general. The experiments are connected with the fact, which has been noticed within the last few years, that the electrical resistance of the metalloids selenium and tellurium diminishes if either of these bodies be exposed to the action of luminous rays; and they have shown that the same phenomenon occurs in the case of metals—that is to say, in the case of platinum, gold, and silver. No other substances have as yet been examined; but the similarity in the behaviour of these three metals renders it extremely probable that sensitiveness to light is not, as one was hitherto inclined to think, a property peculiar to selenium and tellurium, but that we are here dealing with a general property of all metals.

Both these metalloids are ill adapted for proving the phenomenon of sensitiveness to light, inasmuch as they occur in different and partially crystalline modifications, which behave differently as regards the passage of electricity, and can be converted, by means of heat, from one modification into the other. Consequently the effect which is ascribed to light may also, in certain circumstances, be produced in selenium and tellurium by the action of heat; and it is only certain definite modifications of these metalloids which show the reverse behaviour. This, however, is absolutely necessary in order to prove that the observed diminution of resistance is

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[*] Translated and condensed by R. E. Day, M.A., the Owen's College, Manchester.

really due to light, and not to a rise of temperature, which always accompanies any exposure to radiation.   The metals that have been examined differ from selenium and tellurium in this respect, that their resistance invariably increases with the temperature; and when they are exposed to light, the observed change in their conductivity arises from the differential action of two causes, namely the direct action of light and the rise of temperature; and the existence of the first of these may be considered as proved beyond question, if the observed change consists in a diminution of the resistance.

Now, in the case of platinum, gold, and silver, phenomena have been observed which are exactly similar to those already noticed in the case of selenium and tellurium; and their detection was effected by giving these metals such a form that the surface was very large as compared with the mass, so that as much as possible of the mass was exposed to the incident luminous rays.   For the exposure a collimating tube with a slit was employed, a lens like that in use in spectroscopic experiments, and a flint-glass prism of 45° refracting angle, placed in the position of minimum deviation for sodium-light.   The achromatic collimating lens had a focal length of 260 millims., and a diameter of 26 millims.   In front of the slit was a Bunsen burner, the flame of which was coloured in the usual manner by means of a bead of salt on a platinum wire.   When an observation was to be taken without exposure to light, the flame was extinguished, and the slit was covered with a piece of black paper.   In a few cases sunlight was used—sometimes the direct rays, and on other occasions the ray after its refraction through a prism being employed. The actual observations consisted in comparing the measured resistances of the metallic bodies when unexposed, with their resistances when exposed to light.

In order to avoid any risk of error, two different methods of measuring the resistance were employed—the measurement by the Wheatstone bridge arrangement, and the measurement of a resistance according to Weber's method of "damped vibrations."   Among the experiments a few were also made with induced currents; and though they are inconclusive as regards the action of light, they are put forward with the rest because they enable us to draw some interesting inferences concerning the action of a current upon its conductor.   In employing the first method, two equal metallic bodies were simultaneously observed while one and then the other was alternately illuminated; and the ratio of their resistances, under these circumstances, was determined each time independently.   This method of procedure had this advantage,

that the two metallic bodies under comparison were, except
as regards the illumination, continuously under the influence
of identical conditions. The " damping " method did not per-
mit of two metallic bodies being simultaneously observed, and
was therefore employed to determine the resistance of one body
at frequent successive intervals, during which it was alter-
nately in the dark and under the influence of the luminous
rays. This method afforded no security against any possible
influence which alterations of temperature, and other changes,
might have upon observations which succeeded one another
after any considerable interval of time. Nevertheless the ma-
jority of the experiments have been carried out according to
this method, and far more favourable results have thereby been
obtained than with the Wheatstone bridge. The explanation
of this lies in the fact that the " damping " method is much
better adapted for working with electrical currents of very
weak intensity ; so that changes in the resistance, or other
influences, can only very slightly affect the currents which
are employed in the measurement. While in the first of these
methods a Leclanché cell was employed as the electromotor,
in the latter method the currents generated in the coils of
the galvanometer by the oscillations of the magnetic needle
were found to be sufficient ; and it appears that just this par-
ticular difference between the two methods had an important
influence on the results, and therewith also on the applicability
of the methods.

The author then gives a general summary of the results
already obtained by other observers for selenium and tel-
lurium, and then proceeds to describe his own experiments.

Experiments with the Wheatstone Bridge.

In these, as well as in all the following experiments, the
connecting wires were always coated, and the junctions were
carefully soldered. Whenever a connection was made by
means of a mercury-cup, the wires dipping into it were amal-
gamated. The apparatus employed in the first of the methods
was of the form commonly known as the Wheatstone bridge.
The electromotor, a Leclanché cell, was connected directly
with a commutator, and a key, which usually kept the circuit
open, but closed it on being pressed down with the hand. A
second commutator was also employed for the purpose of
interchanging the two resistances whose ratio was required.
With the bridge-wire itself there was included a galvano-
meter, having a copper damper and a reflecting scale ; and
one end of this bridge wire was connected in the usual
manner with a metal slider, movable along the measuring-

wire. The bridge-wire was of German silver, 1150 millimetres long; and to each end there was attached a spiral of copper wire. By this arrangement the sensitiveness of the apparatus became very much greater than it had been when the German-silver wire, originally intended for this purpose, was alone employed. To prevent any changes of temperature by radiation, the two spirals were covered with paper shades during the experiments, and the German-silver wire was protected against any heat radiated from the observer's body by a strip of wood; and before each observation its temperature was rendered uniform by fanning it with a piece of cardboard. The true position of the slider was calculated from two positions differing by one millimetre, on the assumption that within these limits the variations in the deflection of the galvanometer and the position of the slider were proportional to one another. In each of these two positions of the slider, the first deflection of the galvanometer-needle was read off for both directions of the battery-current; and then the second commutator was made use of, and the whole repeated. Eight readings were consequently taken for each determination of the resistance, which was calculated from them by means of a formula determined empirically for this piece of apparatus.

Experiments were first made upon two very thin platinum wires, which were obtained from what is called Wollaston wire, by dissolving off the outer layer of silver with nitric acid. To hold them a box-shaped enlargement was made at that part of the telescope, belonging to the spectroscope employed, where the cross-wires are usually situated. This portion, which was of brass, had four slightly conical holes at right angles to the axis of the telescope; and brass plugs were fitted, air-tight, into these holes, and insulated by means of vulcanite collars. The plugs were connected on the inside with platinum clips, all insulated with vulcanite; and between these clips the two platinum wires were stretched. Their ends were soldered to the clips; and they were placed in the same plane and parallel, symmetrically situated, and at right angles to the axis of the telescope; and by means of the connexions already described they could be separately joined up, from the outside, in an electric circuit. This box containing the wires was closed on the side towards the object-glass by a piece of plate glass, and on the opposite side by an eye-lens, so that it could be connected with an air-pump and exhausted. It appeared afterwards that no change in the experimental results was produced when this space was exhausted to a pressure of 15 millimetres of mercury; and consequently this

arrangement, which, by the way, was intended for a different purpose, was no longer employed. The eye-piece was adjusted so that when the slit in the collimator was illuminated the two platinum wires, and a silver wire stretched horizontally in front of the slit, appeared equally distinct in the field of view. When the wires were vertical, and one of them was illuminated from the slit, then half a turn of the telescope about its axis brought the second wire within reach of the rays, and placed the first wire in darkness. The apparatus was covered with a black cloth, so as to prevent any light but that coming from the collimator and prism from falling upon the object-glass of the observing-telescope. The two platinum wires were each 14 millimetres long, and had each an electrical resistance of about 13 Siemens's mercury units ; their thickness might therefore be calculated at 0·01122 millim., if we assume the law of propagation of the electrical current to be the same in such thin conductors as it is in thicker ones. The wires were connected with the bridge arrangement ; and their resistances were repeatedly compared while one and then the other was alternately illuminated.

The results of these experiments, in which sodium-light was employed, showed most clearly a diminution of resistance by the action of light, and also that, in the course of the experiments, the sensitiveness to light invariably diminished, until finally it was overbalanced by the heating-effect. If the wires were renewed, or whenever any pause occurred in the experiments, the effect of the illumination came out again more strongly. The mean of the results obtained from 98 observations with each wire, gave 0·0125 per cent. as the mean diminution of resistance of each platinum wire owing to the illumination.

Further experiments, according to the same method, were carried out with two strips of gold leaf, the arrangement of which was as follows :—A rectangular plate of glass, about 40 × 70 millims., was coated at opposite ends with tinfoil, so that there was a clear space, 24 millims. wide, right across the glass ; a slip of gold leaf, suitably cut, was then floated on water, and was lifted out by this prepared glass plate in such a manner that it formed a conducting link between the two tinfoil coatings. When the water had evaporated, the gold leaf held firmly to the foil below. If a drop of soft solder be made to fall on the tinfoil it holds firm enough to admit of copper wires being soldered to it; and after both of the tinfoil coatings had thus been fitted with connecting wires, the under side of the glass was covered with black paper, the black side being turned towards the glass. Two such glass plates were

then cemented side by side in an upright position on to a larger glass plate, so that the gold leaves were parallel and vertical ; and then the arrangement was placed in the position previously occupied by the observing-telescope in the apparatus already employed ; and the gold leaves were included in the circuit by means of their connecting wires, just in the same way as the platinum wires had been. A simple slot in the table which carried the whole of the apparatus allowed the glass plate, carrying the gold leaves, to be rapidly slid along, so that one and then the other was alternately within reach of the rays coming from the prism. All the other arrangements remained exactly as before. The length of each of the gold leaves, between the tinfoil coatings, was 24 millims., and its width 9 millims., while its resistance was about 3 mercurial units.

The results obtained in these experiments corresponded exactly with those previously arrived at with the platinum wires ; and we may therefore combine them briefly as follows :—

Platinum and gold exhibit a diminution of resistance in consequence of illumination—the diminution, it is true, being quantitatively of small amount, but qualitatively quite sufficient clearly to establish the reality of the fact.

Experiments with Weber's Magneto-Inductor.

In all the experiments in which the Wheatstone-bridge arrangement was employed the changes of conductivity, due to exposure to light, which could be detected in platinum and gold, were very small, hardly amounting to more than 0·01 per cent. It was impossible to tell from these experiments whether there was any secondary action, connected with the method of observation, which interfered with the luminous effect ; for both the metallic bodies which were examined on each occasion were, throughout the experiment, in exactly the same circumstances, the only difference consisting in their illumination. If any unfavourable influence was present, it would be present invariably, and therefore could not be detected by any comparative experiments. But, as soon as the earlier experiments had shown that there was in all probability a real diminution of resistance in consequence of illumination, it appeared desirable to discover a second method, which would establish the same fact in another way. The first point consisted in separating the metallic bodies under examination ; and henceforward each of them was always experimented upon by itself. One of the gold leaves, already referred to, was first examined, the method of illumination

being the same as before. The electromotor consisted of a Weber's magneto-inductor. The bar magnet, by whose motion the electric currents were induced, was fixed in an upright, cylindrical, wooden frame, at the upper and of which was a string, leading over a pulley to the observer's place, who was thus able to raise or depress the bar magnet at will. The bar magnet was thus made to move in the direction of its axis, which coincided with that of a fixed spiral of copper wire surrounding it. The induced currents were observed by means of a multiplier, wound with thick wire, which had a powerful copper damper and a reflecting scale, its oscillating magnet, which weighed one pound, being suspended by an iron wire from the lid. The resistance of the inductor coil was equal to 2·328 mercurial units, and that of the multiplier 1·223 units; and the two together with the metallic body under examination, and the necessary connecting wires, completed the circuit.

The mode of carrying out an observation is due to Weber, and has been fully described by him [*].

The resistance of one of the pieces of gold leaf was then determined several times in succession, during which it was alternately unexposed and then exposed to the light of a sodium-flame; and the general results of these experiments may be summed up as follows :—

There was a difference in the resistance of about 0·035 per cent. to be accounted for by the exposure to light; but there was also a permanent increase of resistance developed in the gold leaf by the passage of the electric current. For instance, it was found, at the conclusion of one series of experiments, that the resistance of the unexposed gold leaf had increased by 1·25 per cent. of its original value, owing to the passage of the electric current; but on resuming the experiments the next day with the same piece of gold leaf, this after-effect had apparently disappeared, and the gold leaf had regained its original resistance.

Now this increase of resistance during the passage of electricity cannot be regarded as due to any heating by the electric current; for the change went on increasing uniformly during the whole period of the first set of observations, which extended, with several interruptions, over a period of nine hours. Moreover any heat developed in the strip of gold leaf must have been communicated very quickly to the glass plate which was immediately connected with it; and there is

[*] Weber, Elektrodynamische Maasbestimmungen, II., insbes. über Widerstandmessungen, *Abh. d. kgl. sächs. Gesellschaft d. Wissenschaften*, Bd. I. S. 351. 1852.

no doubt that this always took place in the interval between any two consecutive inductive impulses; for the duration of these currents was indefinitely small as compared with the intervals of time which elapsed between them.   During the whole time of the experiments the temperature of the room increased from 13°·7 to 14°·3 C., and therefore could not have been the cause of this observed increase in the resistance. Since the same phenomenon was confirmed by a number of further observations on other days, it appears conclusively proved that

The mere passage of electrical currents increases the resistance of the thin layer of gold by more than one per cent., the effect remaining for the space of about one day.

Experiments by the Damping Method.

In seeking for a further method which would enable the sensitiveness to light of metallic plates, which were being traversed by an electric current, to be observed in a manner free from any disturbing secondary actions, the object to be aimed at was to employ a current of minimum intensity, without, however, diminishing the accuracy of the measurements.   It seemed as if this would be satisfactorily effected by employing the "damping" method due to W. Weber[*], which consists in joining up the conductor, whose resistance is required, with a galvanometer, and observing the oscillations of the galvanometer-needle.   In this way the metallic plate under examination was only traversed by the feeble currents induced in the coils of the galvanometer by the swinging magnetic needle ; and, except in the first few experiments, the metallic plate itself was included in the circuit only at the moment when the deflections of the needle were being read.   The disturbing influence of the current was thus reduced to a minimum: of course it was impossible to get rid of it entirely ; and, moreover, this is not required for qualitative experiments, since, fortunately, it operates in the same way as heating would do, and thus can never be mistaken for the direct luminous effect.

In the majority of the experiments carried out by this method the source of light consisted of the flame of a Bunsen burner coloured with salt, the rays being sent as before through the slit, prism, and lens, onto the metallic plate ; on a few subsequent occasions sunlight was employed.

The experiments were made with gold, platinum, and silver. Gold was employed as before, in the form of strips of gold

[*] Weber, Elektrodynamische Maasbestimmungen, *Abh. der kgl. sächs. Ges. d. W.* Bd. I. S. 374.   1852.

leaf upon glass; platinum, as a very thin film, burnt in on plate-glass, appearing of a grey colour by transmitted light; and silver in layers of varying thickness on plate glass, the layers being of a bluish colour by transmitted light. The platinum held so firmly to the glass, that the thin connecting wires could be soldered to it directly. In the case of silver it was necessary first to cover the points intended for the connexions with a galvano-plastic layer of copper; and then the connecting wires could be soldered on to the layer of copper thus produced. The plates were then fixed to suitable stands, so that the longer side, which was parallel to the line joining the points of connexion, was vertical. The illumination invariably took place on the metallic side.

The dimensions of the gold plates employed were 24×9 millims.; and they had a resistance of 3 mercury units. Those of the platinum plates were about 50×16 millims., with a resistance of 175 units; while the dimensions of the silver plates were about 65×17 millims., with a resistance of 4·853 units.

The experiments consisted in determining, by the above method, the resistance of the metallic plate, first when unexposed, and then when exposed to light. Exposure to light was thus found to produce a diminution of resistance, the maximum of which amounted to 3, 4, and 5 per cent. for the respective resistances of the platinum, gold, and silver plates. These values, moreover, did not by any means remain constant in different experiments, although these were carried out, as far as possible, under similar conditions. On the contrary, whenever, in order to ensure similarity in the external conditions, several experiments were made in quick succession with the same metallic plate, it always became less sensitive to light; in fact, in a few instances the opposite effect was eventually produced, just as if the rays were now only able to generate a rise of temperature. The experiments always succeeded best when the particular metallic plate had not been experimented upon for some days; and when, in such cases, the observations were continued for any length of time, it was very evident that, owing to the duration of the experiments, the sensitiveness to light diminished, but again increased after every pause. The same thing had also been noticed, but less distinctly, in the experiments with the Wheatstone bridge. It might appear open to question, whether the cause of this phenomenon, which has come out in the course of the experiments, is to be sought for in the electrical currents, or in the illumination, since both exerted their influence simultaneously; but, on comparing the experi-

mental methods here employed and the results obtained, we shall see that in the first method, where the more powerful currents were employed, there was, *cæteris paribus*, a much smaller sensitiveness to light; and we may therefore conclude that

The diminution of conductivity produced by the electrical current, which was described above as an electrical after-effect, is accompanied by a diminution in the sensitiveness to light.

In all the previous experiments a coloured gas-flame had been employed as the source of light. In order to test, in this respect, the facts which had been discovered, a few of the thin silver plates were exposed to sunlight, and the change in their conductivity was examined therewith. At first the direct rays of the sun were allowed to pass through the closed window; and the effect produced was found to correspond to that due to a rise of temperature; so that either there was no direct luminous action, or else it was overpowered by the heat developed in the silver and glass by the sunlight. The window was then opened, and by means of a glass condenser the sun's rays were brought to a focus at a short distance in front of the silver plate, since, when focused upon it, they generated too much heat. Under these circumstances, the surprising result was obtained that here again exposure to light produced an increase of 3·7 per cent. in the electric conductivity.

These experiments therefore prove that

" *The metals platinum, gold, and silver, just as was already known in the case of selenium and tellurium, experience, under the action of luminous rays, an increase in their electric conductivity, the magnitude of which, as far as the observations go at present, may amount to from 3 to 5 per cent. of the total conducting-power.*"

Having thus stated the prominent facts, it appears necessary to refute an objection which might possibly be made. It appeared, as was described above, that thin layers of different metals experience, under the action of luminous rays, a change in their resistance which is contrary to that which the resistance of thicker pieces of the same metals was proved to undergo by heating; and hence it was inferred that the phenomenon which had been noticed could not be due to any rise of temperature. It was therefore necessary to show that the electrical resistance of platinum, gold, and silver, when used in these thin layers, increases with a rise of temperature, just as it does in the case of larger pieces. This is, *à priori*, by no means certain, since we are almost entirely ignorant of the molecular constitution of these thin metallic plates. It

therefore seemed desirable to test this point by experiment. For this purpose the Wheatstone bridge was employed, and the resistance of a thin metallic plate was compared with a known copper-wire resistance. In this case the electric current, which was generated by a Daniell's cell, was kept on continuously, and every thing was so arranged that from the commencement there was a small deflection of the galvano-meter-needle. This would necessarily increase whenever the resistance of the lamina increased, while a diminution in the electromotive force of the cell could only slightly diminish the deflection. All the different specimens of thin metallic plates employed in the above experiments agreed with one another in the following phenomena. At first the deflection increased, owing to the effect of the current—considerably in the case of silver, but less so in the case of gold and platinum; then, when a large copper soldering-iron, which had previously been heated to a dull red by a glass-blower's lamp, was brought near the plate under examination, the intense radiation from it produced a considerable increase in the deflection, which, on the removal of the hot iron, diminished again very slowly, at all events much more slowly than did the temperature of the heated metal plate. Hence it follows that, at any rate for ordinary temperatures, the resistance of the thin metal plates increases by heating, just as it does in the case of larger pieces of metal. And, moreover, it appears to follow, from what has just been described, that the heating produces in the thin metal plates an after-effect similar to that produced by the electric current.

The question as to the relative effect of different kinds of light is naturally connected with these results. In order to elucidate this point, different portions of the solar spectrum were employed as sources of light, in a few experiments which in other respects were arranged exactly like the previous ones. Unfortunately the season and the weather have been very unfavourable for the employment of sunlight, and the author has in consequence been unable to carry out many experiments on this point. As far as they go, however, they appear to indicate that, for very thin layers of silver, the maximum luminous effect is in the blue and violet, the minimum in the green; for gold leaf the maximum is in the orange, yellow, and violet, and the minimum in the green and blue. At present the author does not consider himself entitled to give a decided opinion, on account of the paucity of experimental data on this point; but as soon as the weather and other circumstances permitted, he intended to determine accurately, for as many different metals and different thick-

nesses of metal as possible, the luminous effect of the different colours, in order to base thereupon some further inferences. It is clear that experiments in this direction will be of the highest interest as regards our knowledge of optical and electrical phenomena.

The results of this investigation are as follows :—

The property of experiencing a diminished electrical resistance under the influence of luminous rays is not confined to the metalloids selenium and tellurium, but belongs also to platinum, gold, and silver, and in all probability to metals in general.

The electrical current diminishes both the conductivity and also the sensitiveness to light, of its conductor; and both of these, after cessation of the current, gradually acquire their former values.

LXVI. *On Cumulative Resolution.*
By EDMUND J. MILLS, *D.Sc., F.R.S.* *

1. IF a substance, or mixture of substances, combine with itself n times, and each time lose a particular fraction of itself according to a fixed law, it may be said to undergo *cumulative resolution.* The body undergoing cumulative resolution will, in the sequel, be termed the *diapolyte*; and it will be spoken of as cumulatively resolved, or diapolyzed, *with respect to* what it loses (the *apolyte*) in the manner above expressed.

One of the most frequent forms of cumulative resolution is represented by the following general equation :—

$$n\,A_\alpha\,B_\beta\,C_\gamma \ldots - (n-m)A_a\,B_b\,C_c \ldots$$
$$= A_{n(\alpha-a)+ma}\,B_{n(\beta-b)+mb}\,C_{n(\gamma-c)+mc} \cdots$$

When n becomes exceedingly large with respect to m (in which case it will be denoted by ν), the right-hand side of the equation becomes

$$\nu[A_{(\alpha-a)}\,B_{(\beta-b)}\,C_{(\gamma-c)} \ldots].$$

I shall apply the term *cumulate* to a product having this general formula, and thus obtained, and designate it by the special symbol $\bigcirc$, which will always include a numerical value, unless when used as an operator.

In the latter case an expression

$$\bigcirc_{n-m}^{n} X = Y$$

is to be interpreted thus,—"The substance X becomes the substance Y when n units of it lose $(n-m)$ units of apolyte by way of cumulative resolution."

In many cases of chemical action the diapolyte is capable of forming but one cumulate only; but in others a succession of cumulates occurs, each cumulate becoming in turn a diapolyte. Observing that the first cumulate is affected by ν, we have to operate with

$$\nu \left\{ n[A_{(\alpha-a)} \, B_{(\beta-b)} \, C_{(\gamma-c)} \ldots] - (n-m)A_a \, B_b \, C_c \right\},$$

which leads to the second cumulate; &c. &c. Proceeding thus we obtain the following series of cumulates:—

$$\begin{aligned}
\text{⓪} &= \quad [A_\alpha \, B_\beta \, C_\gamma \ldots]; \\
\text{①} &= \nu \, [A_{(\alpha-a)} \, B_{(\beta-b)} \, C_{(\gamma-c)} \ldots]; \\
\text{②} &= \nu^2 [A_{(\alpha-2a)} \, B_{(\beta-2b)} \, C_{(\gamma-2c)} \ldots]; \\
\text{③} &= \nu^3 [A_{(\alpha-3a)} \, B_{(\beta-3b)} \, C_{(\gamma-3c)} \ldots]; \\
&\quad \text{\&c.} \qquad\qquad\qquad \text{\&c.}
\end{aligned}$$

While these differ in composition by a uniform amount, their operator ν proceeds by powers; and the curve representing the relation of ν to their difference is a logarithmic curve. I have not yet met with an instance in which the series eventually vanishes.

It may appear at first sight unreasonable to suppose that bodies of indefinitely high symbolic value, and consequently of indefinitely low specific heat, can possibly exist. We are, however, as yet ignorant of the exact connexion between the specific heat of such bodies and their chemical formulæ; and a great deal of the evidence we possess points to the conclusion that in their case the ordinary law breaks down. The conception, moreover, of a purely expanding series is familiar to chemists[*]. It may be that many or all of the cumulates, at the instant after their formation, undergo katamerization [†]; if so, they can then possess specific heat on the ordinary terms. It is sufficient if some portion of a cumulate is left to undergo the continuous inflictions of the resolving power. In any event the " composition," or symbolic coefficients, of the cumulate would remain undisturbed; and the entire process can therefore be followed by practical analytical operations. I will now consider some typical cases of cumulative resolution.

2. Ammonic Carbonates.—The obscurity surrounding the formation of these salts has been gradually increasing; and the latest experimental researches, while solving some particular problems in connexion with them, have left the more general ones for the most part untouched.

The formulæ of the different ammonic carbonates (omitting

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[*] The paraffin series, $C_n H_{2n+2}$, is an instance, though here the conception is an erroneous one.

[†] *Katamer*, the reverse of a polymer.

hydration water), are derivable, with one exception*, from the equations

$$n\,N_2\,H_8\,CO_3 - (n-1)\,NH_5\,O = N_{n+1}\,H_{3n+5}\,C_n\,O_{2n+1}\ldots, \quad (1)$$

$$\textcircled{1} = \nu[NH_3\,CO_2],$$

and

$$2n\,NH_3\,CO_2 - (n-1)\quad CO_2 = N_{2n}\,H_{6n}\,C_{n+1}\,O_{2n+2}\ldots, \quad . \quad (2)$$

$$\textcircled{1} = \nu[N_2\,H_6\,CO_2]$$

$$\textcircled{2} = \nu^2[N_4\,H_{12}\,CO_2]$$

$$\&\text{c.} \qquad \&\text{c.}$$

$$\textcircled{∞} = \nu^{\infty}[NH_3].$$

This will be evident from the following Tables, in which r denotes the ratio of N to C in the known carbonates, as above referred to.

Table I. (Equation 1).

n	$r_1 = \text{N} : \text{C}$
$-1\dot{6}$	2 : 5
$1\cdot4$	2 : 7
$0\cdot0$	1 : 0
$+1\cdot0$	2 : 1
$1\cdot\dot{1}4285\dot{7}$	15 : 8
$1\cdot\dot{3}$	7 : 4
$1\cdot5$	5 : 3
$1\cdot\dot{6}\dot{3}$	$3\frac{2}{9}$: 2
$2\cdot0$	3 : 2
$3\cdot0$	4 : 3
$\textcircled{1}$ or ∞	1 : 1

Table II. (Equation 2).

n	$r_2 = \text{N} : \text{C}$
$0\cdot0$	0 : 1
$0\cdot\dot{1}\dot{6}$	2 : 7
$0\cdot25$	2 : 5
$1\cdot0$	1 : 1
$2\cdot0$	4 : 3
$3\cdot0$	3 : 2
$4\cdot\dot{1}4285\dot{7}$	$3\frac{2}{9}$: 2
$5\cdot0$	5 : 3
$7\cdot0$	7 : 4
$15\cdot0$	15 : 8
∞	2 : 1

* Rose's nine-fourths carbonate, already recognized as quite isolated in its composition.

According to these Tables, the different ammonic carbonates may be produced either by deammoniating diammonic carbonate, or by decarbonating hydroammonic carbonate. We have also to note the relations

$$r_1 = \frac{n+1}{n},$$

$$r_2 = \frac{2n}{n+1},$$

and

$$r_1 r_2 = 2.$$

These relations are graphically expressed by rectangular hyperbolic curves. A drawing of the equation to r_2 is here
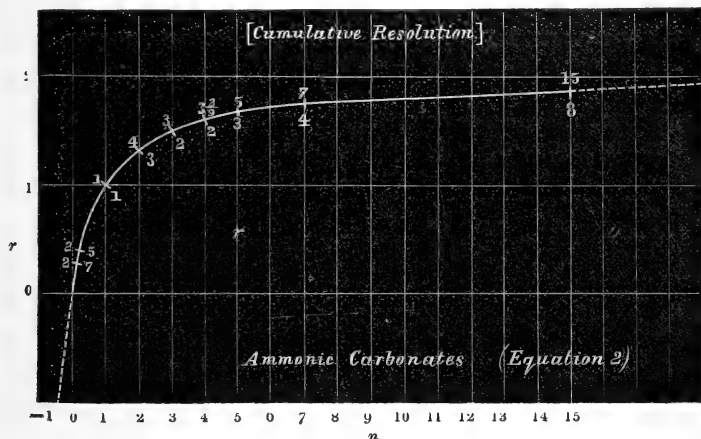
given, where the dots represent portions, at present unknown, of the resulting curve. Considered as far as the first cumulate, all the values of r_2 lie between -1 and $+2$.

3. AMMONIC SULPHIDES.—Aqueous solutions of these bodies are gradually converted by exposure to air into ammonia, water, and sulphur.

$$n\,NH_5S - (n-1)H_2 = N_n\,H_{3n+2}\,S_n \left.\begin{array}{c} \\ \end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad (1)$$
$$① = \nu[NH_3 . S]$$
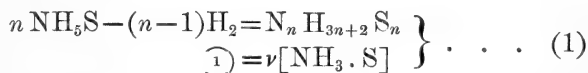
The terms corresponding to $n=1$, $n=2$ are known. The equation to the cumulate suggests the possible presence of a sulphur derivative of hydroxylamine.

$$2n\,N_2\,H_8\,S - (n-1)\{(2n H_3)\,H_2\} = N_{2n+2}\,H_{8n+8}\,S_{2n}. \quad . \quad (2)$$

$$\textcircled{1} = \nu\,[N_2\,H_8\,S_2],$$

$$\textcircled{2} = \nu^2[N_2\,H_8\,S_4],$$

&c. &c.

$$\textcircled{∞} = \nu^\infty[S_\infty].$$

Of this series, the ratios in the first, fourth, fifth, and seventh cumulates are known. The decompositions themselves, however, admit of various modes of representation, according to the starting-point selected.

4. MANGANIC OXIDES.—The derivatives of manganic dioxide can be represented as follows,—

$$2n\,MnO_2 - (n-1)O_2 = Mn_{2n}\,O_{2n+2}.$$

Known ratios of Mn to O correspond to the following values of n; viz. $\cdot5$, 1, $1\cdot2$, 2, 3, and ∞.

FERRIC OXYCHLORIDES.—When ferric chloride is treated with water, it decomposes, eventually producing a body whose composition is very near to that of ferric oxide. Its formula shows that there must be six distinct stages in this continuous process. These are united, for the first time, in the equations

$$n\,Fe_2\,Cl_6\,.\,3\,H_2\,O - (n-1)\,HCl = Fe_{2n}\,Cl_{5n+1}\,H^{5n+1}\,O_{3n},$$

$$\textcircled{1} = \nu\,[Fe_2\,Cl_5\,H_5\,O_3],$$

$$\textcircled{2} = \nu^2[Fe_2\,Cl_4\,H_4\,O_3],$$

&c. &c.

$$\textcircled{6} = \nu^6[Fe_2\,O_3].$$

Ferric compounds of the above nature are somewhat repulsive to analysts; and hardly an attempt at their formulæ appears in systematic works.

5. BISMUTHIC NITRATES.—The action of water on normal bismuthic nitrate is necessarily represented as containing three distinct continuous stages:—

$$n\,Bi_2\,O_3\,.\,3\,N_2\,O_5 - (n-1)N_2\,O_5 = Bi_{2n}\,O_{3n}\,.\,N_{4n+2}\,O_{10n+5}.$$

$$\textcircled{1} = \nu\,[Bi_2\,O_3\,.\,N_4\,O_{10}],$$

$$\textcircled{2} = \nu^2[Bi_2\,O_3\,.\,N_2\,O_5],$$

$$\textcircled{3} = \nu^3[Bi_2\,O_3].$$

In the first stage, ratios are known for $n = \cdot1$ and 1; for the second, $n = 2$; for the third, $n = \frac{6}{5}$, $\frac{5}{4}$, $\frac{4}{3}$, 1, and ∞.

6. SILICATES.—A large number of silicates are derivable

from two silicic hydrates, of which the second is the cumulate of the first.

$$n\,H_4\,SiO_4 - (n-1)\,H_2O = H_{2n+2}\,Si_n\,O_{3n+1}.$$
$$\frown = \nu\,[H_2\,SiO_3],$$
$$\widehat{2} = \nu^2[SiO_3].$$
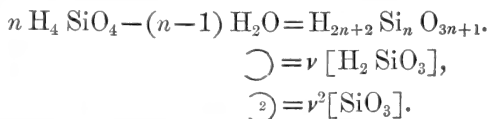
After the first cumulate, the second series is $\nu H_2\,Si_n\,O_{2n+1}$.

For the mixed series,

$$mH_4SiO_4 + nH_2\,SiO_3 - (m+n-2)H_2O = H_{2m+4}Si_{m+n}O_{3m+2n+2};$$

whence

$$\frown = \nu[H_2\,SiO_3\,.\,SiO_2].$$
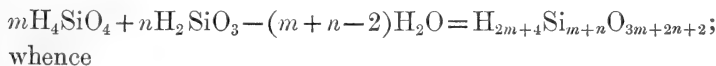
Series 1.—Peridote*, phenakite, zircon, almandine, grossularia, tetrethylic silicate ($n=1$).

Analcime? ($n=1\dot{\cdot}3$).
Okenite ($n=2$).

Magnesite, Labradorite ($n=3$).
Diopside, enstatite, chlorophæite, amphigene, pyrophyllite, talc, emerald, diethylic silicate ($n=\infty$).

Series 2.—Anorthite ($n=\cdot5$).
Fremy's hydrate ($n=1\cdot5$).
Diethylic disilicate ($n=2$).
Doveri's hydrate ($n=3$).

Mixed Series.—Orthose ($n=4,\,m=2$).
Analcime ($n=-1,\,m=5$).
Fuchs's hydrate ($n=\infty,\,m=\infty$).

Before proceeding to the consideration of carbon compounds, it is necessary to consider the relation of homology to cumulative resolution.

7. HOMOLOGY.—If we take any starting-point X, and proceed to form homologues $X\,.\,CH_2$, $X\,.\,2CH_2$, $X\,.\,3CH_2$, &c., we have in general $X\,.\,C_n\,H_{2n}$. When, therefore, n becomes very large, the composition of a member of any homologous series is undistinguishable from that of an olefine. Such composition is moreover attained by a perfectly continuous approach. Take, for example, the fatty alcohols, $C_n\,H_{2n+2}\,O$, which are homologues of water: $\widehat{1}$ is evidently νCH_2, as must also be the case with the aromatic alcohols $C_n\,H_{2n-6}\,O$

* I have taken Wurtz's authority for the formulæ (*Leçons de Phil. Chim.* p. 181. In this place will be found some of the earlier suggestions of a theory).

and any other homologous series. Eventually, then, all homologous series tend to become the same.

The complexity of any member of such series as the above clearly depends on the value of n and on the ratio r of C to H; and these are its only variables. We have, in the series of fatty alcohols,

$$r = \frac{n}{2n + 2},$$

a hyperbolic relation between r and n. A similar relation holds good in all homologous series, excepting that of the olefines, where $r = \cdot 5$, and the complexity (determined by n alone) is altogether linear. *But, this series omitted, it obviously follows that the physical properties of homologous bodies cannot be a linear function of their symbolic value.* Kopp's law, therefore, has no chance of demonstration except in the case of the olefines.

The following equation includes the results of Thorpe and Young[*],

$$C_n H_{2n+2} - \alpha CH_2 = C_{n-a} H_{2n-2a+2}.$$
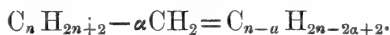
On the whole, it appears that the olefines, not the paraffins, constitute the basis of carbon combinations, to which they are in fact asymptotic.

8. PLANT-PRODUCTS. *Celluloids.*—When a living plant takes up carbonic dioxide and water it loses oxygen, and forms cellulose, cannose, glucose, glucosides, and other products. If we take the equation

$$[(n+1) CO_2 + n H_2 O] - (n+1) O_2 = C_{n+1} H_{2n} O_n,$$
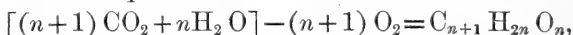
and give successive integral values to n, we obtain the ratios of cellulose, starch, dextrin, or glucosan ($n = 5$), hydric kinate ($n = 6$), cannose ($n = 11$), and glucose ($n = \infty$). The ratios in the cumulate ⌒ are $CH_2 O$. These confirm Debus's law[†], that $\frac{O}{C} = 1$ is a maximum value in the case of alcohols.

Acid Bodies.—The composition of vegetable-acid bodies cannot be represented by a lower formula than $CH_2 O$, nor, according to Debus's[†] law, by a higher one than $C_n H_{x+b} O_{n+b}$, if b stand for the "basicity." In systematic works the general type of the formulæ of these bodies is $C_n H_{2n-2} O_m$, which has the advantage of keeping the coefficient of H necessarily even. The maximum formula is most simply constructed thus,

$$[n CO_2 + (n-p) H_2 O] - O_{p+x} = C_n H_{2n-2p} O_{3n-(2p+x)},$$
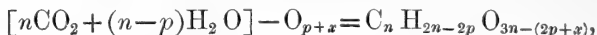
* Proc. Roy. Soc. vol. xx. p. 488.
† Chem. Soc. Journ. vol. xix. p. 256.

whence $\widehat{\mathrm{1}} = \nu(\mathrm{CH}_2\,\mathrm{O}_3)$. The ratio $\dfrac{\mathrm{O}_3}{\mathrm{C}}$ in this cumulate is the highest permissible in any formula, according to the law referred to, while H_2 is a minimum. It appears, then, that $(\mathrm{CH}_2\,\mathrm{O})$ and $(\mathrm{CH}_2\,\mathrm{O}_3)$ are the extremes of composition for acid vegetable bodies; that is, it ranges between that of glucose and hydric carbonate. Obviously also the symbolic ratio $\dfrac{\mathrm{H}}{\mathrm{C}}$ cannot exceed 2, nor x exceed $3n$. Moreover, since these bodies are, as a series, much less deoxidized than the glucoses and sugars, it is natural to find them the precursors of the latter in plant-life.

Glucosides are intermediate between celluloids and acid bodies.

9. *Fermentation Alcohols.*—During the fermentation of glucose (or its isomers or polymers) by means of yeast, it is found that not only is ethylic alcohol present, but several of its higher homologues; methylic alcohol, however, is not found. Carbonic dioxide is given off in abundance. The following equation appears to me to connect these facts continuously,—

$$2n\,\mathrm{CH}_2\,\mathrm{O} - 2\,\mathrm{CO}_2 - (n-3)\,\mathrm{O}_2 = 2\,\mathrm{C}_{n-1}\,\mathrm{H}_{2n}\,\mathrm{O}$$
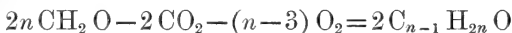
(which is the same as $\mathrm{C}_n\,\mathrm{H}_{2n+2}\,\mathrm{O}$, the general formula for a fatty alcohol). For $n=0$, the result is O_2; for $n=1$, $-2\,\mathrm{H}_2\,\mathrm{O}$; for $n=2$, methylic alcohol; for $n=3$, ethylic alcohol, &c. &c. In all values of n up to 3, the process is one of oxidation, as is evident from the consequent values of $\{-(n-3)\mathrm{O}_2\}$; hence the equation is irrational, as far as the formation of alcohols is concerned, until n is equal to or greater than 3. The condition for the production of alcohols is then attained; and the first to be formed is necessarily ethylic.

It appears that a very small percentage of the glucose present is katamerized, in part hydrogenized, and in part oxidized during fermentation. In this manner a little glycerine $(3\,\mathrm{CH}_2\,\mathrm{O} + \mathrm{H}_2)$ and hydric succinate $(4\,\mathrm{CH}_2\,\mathrm{O} - \mathrm{H}_2)$ are formed.

Since $\widehat{\mathrm{1}} = \nu\mathrm{CH}_2$, it appears that the composition of the series of fatty alcohols lies between O_2 and CH_2. And the same principle that presides over the formation of plant-products conducts us continuously through the genetic relations of fermentation alcohols.

10. POLYGLUCOSIC and PLYGLYCERIC ALCOHOLS.—In the case of polyglucosic acohols, we have, according to Watts[*],

$$n\mathrm{C}_6\,\mathrm{H}_{12}\,\mathrm{O}_6 - (n-1)\,\mathrm{H}_2\,\mathrm{O} = \mathrm{C}_{6n}\,\mathrm{H}_{10n+2}\,\mathrm{O}_{5n+1}.$$

[*] Fownes's 'Manual of Chemistry' (1868) p. 683.

Hence ①$=\nu C_6 H_{10} O_5$;   ⑥$=\nu^6 C_6$. Water and carbon are the extremes of composition for this series.

For polyglyceric alcohols, according to the same authority[*],

$$nC_3 H_8 O_3 - (n-1) H_2 O = C_{3n} H_{6n+2} O_{2n+1}.$$

Hence ①$=\nu C_3 H_6 O_2$ or glycide; ③$=\nu^3 C_3 H_2$. $H_2 O$ and $C_3 H_2$ represent the two extremes of this series.

Caramels.—By a similar process, applied to cannose, the series of caramels is formed,

$$nC_{12} H_{22} O_{11} - (n-1) H_2 O = C_{12n} H_{20n+2} O_{10n+1}.$$

Of its members, ①$=\nu C_{12} H_{20} O_{10}$, ②$= \nu^2 C_{12} H_{18} O_9$ (caramelane), ⑥$= \nu^6 C_{12} H_{10} O_5$, and ⑦$= \nu^7 C_{12} H_8 O_4$ (caramelene) are known. Caramelin seems to be exactly intermediate between ⑤ and ⑥.

11. ETHERIFICATION.—In the case of the fatty alcohols, it is usual to form ethers by what, in effect, is a dehydrating process. This is accurately represented by the equation

$$2C_n H_{2n+2} O - H_2 O = C_{2n} H_{4n+2} O,$$

whence ①$=\nu C_2 H_4$. In conformity with this result, we find that the action of oil of vitriol on fatty alcohols, when pushed to an extreme, yields olefines, but not methylene. Here also we find striking evidence of the correctness of the theory of cumulates as enunciated in (1).

12. CONCLUSION.—The process of cumulative resolution, of which a brief sketch has been given in the foregoing sections, is one of very general prevalence in chemical operations. In the particular form of it there illustrated, the transition from one compound to a successor in multiple proportion is effected in hyperbolic paths, whose successive points at infinity lie on a logarithmic curve. Such points at infinity are actually realized as conditions of unstable equilibrium, under which a new ordinal compound commences to take its rise.

Having regard to the fact that the same theory that applies to plants is also found to regulate the deportment of ordinary reagents and mineral substances, we must so far regard the latter as living beings.

Anderson's College, Glasgow.

[*] Fownes's 'Manual of Chemistry' (1868), p. 666.

LXVII. *On the Thermal Phenomena of the Galvanic Pile, and Electromotive Forces.* By E. EDLUND.

[Concluded from p. 441.]

WE will now apply to Favre's experiment the theoretical representation No. 2 ; and we will denote by E the electromotive force of the series, and by e the electromotive force of polarization in the voltameter, and assume that the resistance in the voltameter can be neglected in presence of the resistance of the series and rheostat.

(a) *Calorimeter No.* 1 *including the series and the rheostat, without a voltameter.*—While, in each element, an equivalent of zinc dissolves, the chemical processes develop a quantity of heat which we will call k; at the same time the electromotive force consumes a quantity of heat which is equal to $23900 \frac{E}{d}$, wherein d signifies, as before, the electromotive force of a Daniell element. The quantity of heat generated in the series and the rheostat is $gw = \frac{M}{m} E$. For the Daniell element we have $23900 = \frac{M}{m} d$, from which we get $gw = 23900 \frac{E}{d}$. Putting together the action of these three sources of heat, we obtain the quantity k of the chemical heat, which alone, in this case, determines the deflection of the calorimeter.

(b) *Calorimeter No.* 1 *including the series, the rheostat, and the voltameter.*—The quantity of heat expended for the chemical decompositson in the voltameter may be named a. The total amount of chemical heat is therefore now $k-a$. In the series the quantity of heat consumed is $23900 \frac{E}{d}$; but in the voltameter a quantity of heat $23900 \frac{e}{d}$ is generated, because the electromotive force of polarization acts in the opposite direction to the current. The galvanic heat developed in the conductors is $= \frac{E-e}{d} \times 23900$. Putting together the action of all these sources of heat, we get the sum $k-a$, which in this case the calorimeter indicates.

(c) *Calorimeter No.* 1. *containing the series and the rheostat, and the calorimeter No.* 2 *containing the voltameter.*—In No. 1 a quantity of chemical heat $=k$ is developed ; but during the same time a quantity of heat $= 23900 \frac{E}{d}$ is consumed by the

electromotive force.   Because it can be assumed that the entire resistance is included in No. 1, a quantity of galvanic heat $= \dfrac{E - e}{d} \times 23900$ is developed there.   The sum of the actions of these three sources of heat becomes $k - 23900 \dfrac{d}{e}$. If this quantity of heat be compared with that which was obtained in the first case (a), the comparison shows that, through the insertion of the voltameter, the calorimeter No. 1 suffered a loss of heat $= 23900 \dfrac{e}{d}$, although it cannot have given up its heat to that calorimeter.   A source of heat is produced in No. 2 by the electromotive force of polarization acting in the opposite direction to the current.   The heat thus produced is $= 23900 \dfrac{e}{d}$ units—that is, equal to the amount of heat lost by calorimeter No. 1.   But the quantity a of heat is consumed by the chemical decomposition.   The change of heat in calorimeter No. 2 therefore becomes $23900 \dfrac{e}{d} - a$.

In Favre's·experiment it was shown that the loss of heat in $\left(23900 \dfrac{e}{d} \right)$ in calorimeter No. 1, when the voltameter contained water, amounted to 54235 units.   The consumption a of heat in the decomposition of the water amounted to 34204.   The difference between these two numbers is 20031.   Calorimeter No. 2, as mentioned above, indicated for this difference 20335 heat-units.

When the voltameter contained the copper solution, the loss of heat in calorimeter No. 1, according to Favre's experiment, came to 38580 units.   The heat-consumption on the decomposition of the salt of copper amounted, according to the experiment, to 26568.   The difference between the two numbers becomes therefore 12012.   Calorimeter No. 2 should consequently have shown a production of 12012 units of heat ; but the direct reading gave 12445.   The difference, however, between calculation and observation is not too great to be ascribed to unavoidable errors of observation.   A reason why the voltameter No. 2 indicated in both cases a somewhat higher production of heat than the calculation required is, that in the calculation the resistance of the voltameter was supposed equal to *nil*, which cannot be quite correct.   There was therefore really present in the voltameter a small source of heat which was not taken into account, namely the galvanic heat-development ; and from this was doubtless chiefly derived

the small difference mentioned. Consequently calorimeter No. 2 could not but indicate, as it actually did, a somewhat greater development of heat ; and hence we are perfectly justified in asserting that theory and experiment are, within the limits of errors of observation, closely accordant with each other.

6. Sir W. Thomson and, subsequently, Bosscha made the following experiment * :—Two equal voltameters are filled with the same quantity of sulphuric-acid water, and then successively inserted in the circuit of a powerful galvanic series. The electrodes in one of the voltameters both consisted of platinum ; but in the other the positive electrode only was of this metal, while the negative consisted of amalgamated zinc. Although the same current passed through both voltameters, and consequently an equal amount of chemical decomposition took place in each, yet the temperature rose much more rapidly in the voltameter which has zinc for the negative electrode than in the other. Bosscha regards the unequal elevations of temperature in the two voltameters as occasioned by the power of the zinc to transfer hydrogen from the active to the ordinary state being different from that which platinum possesses in this respect.

I have repeated this experiment, and obtained the same result. By applying the mode of representation No. 2, it is easy to find the cause of the different elevations of temperature. There is in each of the voltameters an electromotive force which acts in the opposite direction to the current of the series. In the voltameter of which both the electrodes consist of platinum this force originates exclusively from the polarization of hydrogen and oxygen. In the other voltameter oxygen is separated at the platinum, and hydrogen at the zinc ; but, besides the polarization hence arising, this voltameter acts also as a pile, in the opposite direction to the current. That the platinum-zinc vessel develops a greater counterforce than the other vessel, one can easily convince one's self by first inserting the one vessel in the circuit of the current, and then withdrawing it and putting the other in its place. One then finds that the current of the series is considerably more enfeebled by the platinum-zinc vessel than by the other. The quantity of heat arising from the current passing in the opposite direction to the electromotive force which acts in the two voltameters, must on that account become greater in the platinum-zinc vessel than in the other.

From what has been here alleged it follows that the thermal phenomena in the pile, as well as in the voltameter, can be

* Pogg. *Ann.* vol. ciii. p. 487.

accounted for in a simple manner according to the mode of representation No. 2, without it being necessary, as in No. 1, to take to our aid causes the magnitude of the actions of which cannot be directly determined, and which, moreover, closer examination shows, lead to results that are incompatible with one another. We might have carried out several more calculations and comparisons of this sort, if only the treatises which communicate the results of the experiments instituted with the view in question had contained the necessary data; but what we have given from older experiments may be sufficient for the present purpose.

7. By the following experiment which I made, an unequivocal proof is obtained that heat is developed when the current passes through an electromotor in the opposite direction to the acting electromotive force.

Two equal test-tubes of glass (A and B) were filled with the same volume of water containing sulphuric acid; and in each of these tubes two platinum wires and a thermometer were immersed. All four wires were cut off from the same piece; and the two thermometers were also alike. The wires and the thermometers passed through the corks with which the tubes were closed, and were thereby kept undisturbed in position during the experiment. One of the wires in the tube A was connected with one pole of a Bunsen series of from four to six elements; the other wire in the same tube was put into connexion with a commutator which I have described in a previous memoir. The principal constituent of this commutator consists of a circular disk, upon the periphery of which two brass springs slide. When one of these springs is connected to one end, and the other spring to the other end of a conductor, the current in this circuit is incessantly changing its direction as soon as the disk is set in rotation*. A conducting-wire united one slide-spring of the commutator to one of the platinum wires in the tube B; from the other platinum wire in the same tube went another conducting-wire to the other slide-spring; and, lastly, the other pole of the series was connected with the commutator. If now the disk of the commutator was put in rotation, the currents which went through the liquid in tube B incessantly changed their direction. When the disk made one rotation in a second, the number of current-changes was 24 in the same time. The same currents went also through tube A; but here they all had the same direction. The currents which traversed the two tubes were therefore in number, intensity, and duration perfectly equal; the sole difference consisted in this—that in

* Pogg. *Ann.* vol. clvi. p. 251.

tube A all went in the same direction, while in tube B a succeeding current received from the commutator an opposite direction to that of the immediately preceding one.   In this way no perceptible polarization was produced, in tube B, upon the platinum wires.   If the current of the series was not too strong, no evolution of gas was observed in tube B ; but if its intensity was very great, it is true that gas-evolution was perceived in B, but this was always much inferior to that which took place in tube A.   The current produced the same amount of chemical decomposition in both tubes ; but in tube B the separated gases for the most part combined again to form water, whereas in tube A no such recombination took place.   From this it follows that chemical decomposition must have occasioned a greater cooling of the liquid in A than in B.   On the other hand, the galvanic heating occasioned by the passage of the current must have been equal in the two tubes, because the resistance and the current-intensity were the same in both ; for the circumstance that the polar wires in tube A were coated with a thin layer of gas could not exercise any sensible influence upon the resistance.   In consequence of this there was ground for supposing that the rise of temperature in tube A would be less than in tube B ; but the contrary was observed : the temperature rose much more quickly in A than in B ; and this was still the case when the tubes were exchanged or the experiment varied in other respects.   The difference between the two thermometers increased with the current-intensity.   To give an idea of the amount of this difference, it may here be added that, in an experiment in which six Bunsen elements were employed, within five minutes the temperature rose in the tube B from 16° to 26°, in the tube A from 16° to 30°.   The difference therefore amounted in this case to 4°.

The cause of the greater heat-development in tube A cannot possibly be any other than this—that in that tube there was an electromotive force which acted in the opposite direction to the current of the series, while the incessant alteration of the currents prevented the rise of such a force in the tube B.

By the foregoing the theorem is proved:—that if a galvanic current traverses an electromotor in the same direction as that in which the electromotive force acts, a quantity of heat is consumed which is proportional to the electromotive force multiplied by the intensity of the current ; and that if the current has a contrary direction, exactly as great a quantity of heat is generated.

The experimental trials which have been instituted respecting the thermal phenomena in the hydroelectric piles have

consequently led to the result that these electromotive forces consume heat to form the current ; they are therefore in this respect similar to the thermoelectric forces, which likewise form currents by the consumption of heat. In the same manner do those induced electromotive forces behave which arise through the diminution of the intensity of the inducing current. It is demonstrated by experiments that through induction of this sort a loss of heat is produced in the path of the inducing current, which is just equal to the heat-generation produced by the induced current in its path *. If, on the other hand, the induction takes place while the inducing current of constant intensity is brought nearer to or removed further from it, the mechanical work employed for diminishing or increasing the distance is consumed in the formation of the current. Finally, as to the electromotive force met with in the electrical discharge spark, in it a portion of the *vis viva* which is contained in the shock of the discharge, and which is proportional to the mass moved multiplied by the square of the velocity, is transferred to a greater mass possessing less velocity †.

In a previous memoir, I have shown that the laws of galvanic induction can be theoretically deduced if we start from the principle that electromotive force consumes heat for the formation of currents ‡. This principle has proved correct also in the case of the current performing external work.

§ II.

8. From what has been brought forward in the preceding section it follows that it cannot be shown, without arbitrary assumptions, that the heat which the chemical processes develop in a hydroelectric pile gives a measure for the electromotive force. In my opinion, therefore, there is no warrant for the assertion that the chemical and electromotive forces stand in immediate connexion with each other ; while, on the contrary, the results obtained by experiment can very easily be explained if it be admitted that the electromotive force of the pile uses up heat for the production of the current. The only objection of any consequence that was formerly urged against the validity of the contact theory consisted in this— that it could not be shown what the galvanic current was formed from ; for of course it could not be generated out of nothing. This objection, however, lost its force as soon as it could be shown that the current is formed at the expense of heat. This must nevertheless not be interpreted as signifying

* Pogg. *Ann.* vol. cxxiii. p. 193.   † Ibid. vol. cxxxix. p. 355.
‡ Ibid. vol. clvii. p. 102.

that contact electromotive force can be absolutely independent
of the chemical affinity between the substances which are in
contact with one another.

Since, in order to explain the thermal phenomena in the
hydroelectric pile, we are compelled to assume that the elec-
tromotive force found in it consumes heat when a current
passes through in the same direction as that in which the
force acts, but generates heat if the current has the contrary
direction, we must also admit that the same thing must also
happen at the surface of contact between two metallic con-
ductors at which an electromotive force is present, and, there-
fore, that the explanation which I have given of Peltier's
phenomena *, may be the only true one.   I will therefore not
dwell further upon the objections which were raised against
the correctness of that explanation.   In connexion therewith
I have sought to prove that the thermoelectric force arises
from the dependence of the contact electromotive force on
temperature †.   In consequence, however, of the reasons
alleged by Prof. Clausius ‡ this view of mine has not met
with general recognition.   I will therefore take leave here to
subject Prof. Clausius's arguments to a brief examination.

9. In reference to the origin of thermoelectric currents,
Clausius urges, in the memoir cited, that it is in the
highest degree improbable that the attraction of the mate-
rial molecules for electricity should change with the tem-
perature ; and if this were not the case, the distribution of
heat would have no influence at all upon the distribution of
electricity.   But, continues Clausius, even if it be conceded
that the attraction of matter for electricity is really dependent
on temperature, still no thermoelectric current can thence
arise ; for if material molecules at different temperatures pos-
sessed different amounts of attraction for electricity, variously
hot parts of the same conductor would behave like conductors
of different material at the same temperature.   That is to
say, in other terms, a thermoelectric ring consisting of two
different metals, for example, the two soldering-places of
which have different temperatures, would behave just like a
ring consisting of several different metals with all the contacts
possessing the same degree of heat.   But it is known that
such a ring gives no current, because the electromotive forces
in it counteract one another.

For my part, I cannot assent to this assertion of Professor
Clausius.   On the contrary, according to my conviction, every
thing tends to show that the attraction of material for electrical

* Oefversigt af Vetenskaps Ak. Förh. 1869, Vet. Ak. Handl. Bd. ix.
No. 14.
  † Ibid.   ‡ Pogg. Ann. vol. xc.

molecules changes with the temperature, and that a conductor, of which one part is hotter than another, does not behave just like a conductor composed of two chemically different metals both possessing the same temperature. For this the following reason may be given :—

We will imagine two different conductors M and N in contact with one another, and that M exerts a greater attraction than N upon an electrical molecule m. It may here be remarked *en passant* that the attraction exerted by a body upon an electrical molecule in its immediate vicinity must depend not merely on the nature of the molecules of the body, but also on their relative position and distance from one another. We will now assume that the molecule m is situated on the same side of the surface of contact as N, and at the distance r from the same surface, and that r does not exceed the distance within which the molecular forces can operate. It is then evident that the force of attraction which tends to carry the molecule m to the surface of contact is increased when the distance from that surface is diminished ; the force reaches its maximum when m is at the surface itself, but again decreases as soon as m removes from it and penetrates into M. Lastly, the force becomes insensible when the molecule m has penetrated into M so far that the distance from the surface of contact attains the magnitude of the action-radius of the molecular attraction-forces. Moreover, the law according to which the attraction increases when m, being in N, is approaching the contact-surface must be similar to the law according to which the attraction decreases when m is in N, and is moving from the surface mentioned. The action of the attraction which in the time $2t_0$ tends to bring the molecule m to the surface of contact, can then be expressed by $\dfrac{2at_0}{r^n}$, in which n denotes the power according to which the attraction decreases when the distance becomes greater, and a is a constant. The expression $\dfrac{2at_0}{r^n}$ denotes the increment of quantity of motion which the molecule m would receive if it were free and could move, and if a determinate invariable force $\dfrac{a}{r^n}$ acted upon it during the time $2t_0$. If we now imagine the molecule m at the distance $r-\rho$ from the contact-surface, this attraction during the time t_0 will be expressed by $\dfrac{at_0}{(r-\rho)^n}$, and at the distance $r+\rho$ the attraction during an equal period by $\dfrac{at_0}{(r+\rho)^n}$. Therefore, if the molecule m be first during the time t_0 at the distance $r-\rho$

and afterwards during exactly as long a time at the distance $r + \rho$, the molecule will receive during the total time a quantity of motion equal to

$$\frac{at_0}{(r - \rho)^n} + \frac{at_0}{(r + \rho)^n} = \frac{2at_0}{r^n} + \frac{n(n + 1)}{r^{n+2}} at_0 \rho^2,$$

where higher powers of ρ may be neglected, because it is assumed that ρ is an extremely small quantity. We gather from this that the attraction which during the time $2t_0$ tends to bring the molecule nearer to the contact-surface is greater if the molecule is during one half of the time at the distance $r - \rho$, and during the other half at the distance $r + \rho$, than if during the entire period the molecule is at the distance r. If, then, the molecule m describes in the time $2t_0$ a closed path about its initial position of equilibrium which is divided into two equal parts by a plane passing through the molecule's position of equilibrium and parallel to the contact-surface between M and N, the variation of the distance from the contact-surface, or ρ, is a function of the time t, and for corresponding points in each half of the path is equal in amount, although with contrary signs. The augmentation of the quantity of motion which arises *in consequence of the movement* of the molecule m about its position of equilibrium can then be expressed by

$$\frac{n(n + 1)}{r^{n+2}} a \int_{t=0}^{t=t_0} \rho^2 dt.$$

If the molecule m in the same time, $2t_0$, describes about the position of equilibrium a path of like form with the preceding, but with a tangential velocity p times as great in every point, the distance at every point from the position of equilibrium is also p times as great as at the corresponding point in the former path. But the variations in the distance of the molecule from the contact-surface also then become p times as great as before, and can therefore be expressed by $p\rho$. The augmentation of the motion which results from the movement of the molecule about its position of equilibrium becomes, consequently, in this case

$$\frac{n(n + 1)}{r^{n+2}} ap^2 \int_{t=0}^{t=t_0} \rho^2 dt.$$

The total quantity of motion in question, A, which the molecule m acquires while it describes in the same given time paths of like form with different velocities about its position of equilibrium, can therefore, B and C denoting constants, be expressed by

$$A = \frac{1}{r^n} \left(B + C \frac{p^2}{r^2} \right).$$

What is here advanced can of course be applied to any molecule whatever that is sufficiently near to the contact-surface.

We now assume that the electric fluid consists of the lumiferous æther, and that the heat of a body is caused, at least a given portion of it, by the vibrations of the æther molecules about their positions of equilibrium [*]. The quantity of heat contained by the body is then determined by the sum of the *vis viva* of the molecules; and its temperature, reckoned from absolute zero, can be regarded as proportional to this sum; for the deviation that may take place does not affect the final result. If the body at an ordinary temperature receives an inconsiderable elevation of its degree of heat, *e. g.* from $10°$ to $20°$, there are no physical grounds for the assumption that the period of a vibration of the molecules is thereby altered; on the contrary, it must remain unaltered, because the elasticity-forces, on which the motion and the rate of vibration depend, cannot thereby undergo any sensible change. On the other hand, the amplitudes of the molecule-vibrations are augmented by the small rise of temperature, while the path remains of the same form as at the lower temperature. The vibration-paths of the æther molecules also satisfy the condition stated, that they be divided into two equal halves by a plane which passes through the position of equilibrium of the molecules and is parallel to the surface of contact between M and N. But if the increment of temperature is great, experiment shows that not only is the time of a vibration of the molecules lessened, but also the molecules of the body change their position and increase their distance from each other. Therefore it is only for slight increments of temperature that the above conclusion can be applied to the calculation of change of attraction. It is self-evident that p^2 is proportional to the *vis viva* of the æther molecules, and therefore that, if T denotes the absolute temperature of the body, we can write $p^2 = f\mathrm{T}$, where f signifies a constant. If A_0 is the amount of the attraction at the temperature T_0, and A_1 at the somewhat higher temperature T_1, we get

$$A_1 - A_0 = D(T_1 - T_0),$$

in which D is a constant which depends *inter alia* on the

[*] The heat possessed by a body is doubtless occasioned also in part by the vibrations of the body's own molecules; yet it is readily seen that the variation of the attraction which results from the *vis viva* of the material molecules need not be taken into account, because the action of this variation, when a thermoelectrical ring is in question, becomes, for the entire ring, equal to *nil*.

difference between the attractions exercised by M and N upon the same electrical molecule.

If the contact-surface at ordinary temperature receives a slight elevation of its degree of heat, the attraction-forces which tend to bring the electric fluid to the conductor which exerts the strongest attraction increase ; and this augmentation of attraction must be approximately proportional to the increment of temperature if this is not too great.

We will now suppose that, for example, the conductor M is divided into two, $M_{/}$ and $M_{//}$, of which $M_{/}$ has a higher temperature than $M_{//}$, but the excess is so slight that the material molecules in the part $M_{/}$ do not on account of it sensibly alter their relative position and distances from one another. M therefore differs from $M_{//}$ only in this—that the molecules of the former are conceived to be in stronger vibration than those of the latter ; but this circumstance need not be taken into consideration, because, as above remarked, the action from this becomes, on the whole, equal to *nil* for the thermoelectric ring. The constant D in the foregoing formula is, in this case, equal to *nil* ; and consequently, for the contact between the warmer and the cooler portions of M, we get

$$A_1 - A_0 = 0.$$

If the excess of temperature possessed by $M_{/}$ is but slight, then $M_{/}$ and $M_{//}$ do not, as regards their attraction, behave toward an electrical molecule at all like two different metals. Consequently different distribution of temperature in one and the same conductor cannot put the electric fluid in motion. If, on the contrary, $M_{/}$ has a temperature so considerably higher than $M_{//}$ that its molecules are caused by it to sensibly change their positions of equilibrium and their distances from one another, the relation may take another form. It may then happen that the attraction exerted by $M_{/}$ upon on electric molecule situated in the vicinity of the contact-surface will be no longer just equal to that which issues from $M_{//}$; and therefore the part $M_{/}$ will, in respect of its attraction, behave toward an electric molecule in some sort as if it were of a different material from $M_{//}$.

In a thermoelectric ring, therefore, consisting of two different metals M and N, of which one of the junctions has a considerably higher temperature than the other, a thermoelectric current must arise with an intensity approximately proportional to the temperature-difference—a result which is known to agree with experiment. When the one junction receives a considerably higher temperature than the other, the *vis viva* of the æther molecules at the junction is proportionally

increased.   Now this may take place by the vibration-period
diminishing while the amplitudes remain unaltered or even
become smaller than before.   When the last-mentioned takes
place, the action of the attraction-force which tends to carry
the electric molecules from N to M becomes feebler at the
higher than at the lower temperature ; and hence there results
in the thermoelectric ring a current running in the contrary
direction to that current which is produced by a smaller tem-
perature-difference between the two junctions.   Herein is to
be seen the cause of the known alteration of the current-
direction shown by several metallic combinations with great
difference of temperature between the junctions *.

   If, therefore, it be admitted that the electric fluid consists
of the luminiferous æther, it may be regarded as proved that
contact electromotive force must vary with the temperature—
a relation which, besides, has been, in my opinion, practically
demonstrated by Le Roux †.

Stockholm, June 3, 1876.

LXVIII. *Note on the Steam and Hoar-frost Lines of Water-
    Substance.*   By ROBERT E. BAYNES, *M.A., Senior Student
    of Christ Church, Oxford, and Lee's Reader in Physics* ‡.

IN the ' Report of the British Association ' (1872), ii. p. 24,
    and the ' Proceedings ' of the Royal Society (1873), xxii.
p. 27, Prof. James Thomson shows that the steam and hoar-
frost lines of water-substance (to use Prof. Maxwell's terms for
the curves $p=f(t)$, $p'=f'(t)$, which represent the maximum
tension of steam in contact with water and ice respectively)
are not continuous, as M. Regnault thought, but intersect at
an angle which is re-entrant downwards.   Two proofs of this
are given:—one, that in any other case the perpetual motion
might subsist ; the other, that the values of $\dfrac{dp}{dt}, \dfrac{dp'}{dt}$ at 0° C.
are theoretically in the ratio of 1 to 1·13, while M. Regnault's
empirical formulæ give the ratio 1 to 1·10.

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   * In consequence of Prof. Clausius maintaining that thermoelectric
force cannot arise from the contact-electromotive force changing with
the temperature, F. Kohlrausch has (Pogg. *Ann.* vol. clvi.) set up the
hypothesis that these forces are not in any way connected with one ano-
ther, but that it is much more probable that the thermoelectromotive
force is caused by the heat-current from the hotter to the cooler contact,
and is proportional to this heat-current.   M. Kohlrausch, however, has not
shown the physical necessity or the possibility of such a connexion between
these phenomena.
   † *Annales de Chimie et de Physique*, (4) t. x.
   ‡ Communicated by the Author.

The discontinuity and intersection of these lines was pointed out nearly twenty years ago by M. Kirchhoff, in Poggendorff's *Annalen*, 1858, ciii. p. 206, in a memoir entitled "Bemerkung über die Spannung des Wasserdampfes bei Temperaturen, die dem Eispunkte nahe sind." As this memoir seems to have dropped out of sight (for I have never seen any reference made to it), it may not be without advantage to resume its argument.

The assumption is made that there is some temperature at which both ice and water can exist, and at which the steam formed over ice is exactly identical with that formed over water. M. Kirchhoff has taken this temperature as $0°$ C., though it must be higher by about $0°\cdot0074$, as the pressure is only about $4\cdot6$ millims. instead of 760 millims. of mercury. The difference is negligible; and the temperature will be taken as $0°$ C., or $273°\cdot7$ on the absolute scale, and denoted by t_0. The argument, then, is, that when unit mass of ice at this temperature is directly transformed into vapour, its intrinsic energy is increased by exactly the same amount as if the transformation were first into the liquid and then into the vapour state.

Now, during vaporization at temperature t from the solid state, the heat L' spent is given by

$$JL' = (v'-u')t\frac{dp'}{dt},$$

where J is the mechanical equivalent of heat, t the absolute temperature, u' the specific volume of ice, and p', v' the pressure and specific volume of steam over ice. But external work equal to $p'(v'-u')$ is done; so that the increase of intrinsic energy is $(v'-u')\left(t\frac{dp'}{dt}-p'\right)$. Hence, in the given case, the increase is $(v'_0-u'_0)(t_0\varpi'_0-p_0')$, the suffixes denoting values for t_0, and ϖ' being written for $\frac{dp'}{dt}$.

Similarly, in the second process, the increase of intrinsic energy during vaporization from the liquid state is

$$(v_0-u_0)(t_0\varpi_0-p_0),$$

where u_0 is the specific volume of water at t_0, and p_0, v_0 the pressure and specific volume of steam over water. Also, during the liquefaction, the increase of intrinsic energy is $Jl_0+p''_0(u'_0-u_0)$, if l_0 is the heat required for the change of state at the pressure p''_0; for during the contraction the work

$p''_0(u'_0 - u_0)$ is done *on* the mass.   Hence

$$(v'_0 - u'_0)(t_0 \varpi_0' - p'_0) = (v_0 - u_0)(t_0 \varpi_0 - p_0) + Jl_0 + p''_0(u'_0 - u_0).$$

But, by the above assumption, $v'_0 = v_0$ and $p'_0 = p_0 = p''_0$; also u'_0 and u_0 may both be neglected in comparison with v_0, of which they are less than the 200,000th part.   The equation in its simplified form therefore becomes

$$\varpi' - \varpi = \frac{Jl_0}{v_0 t_0}.$$

A numerical value for this difference is thus found : if the pressure of 1 millim. of mercury be taken as unit pressure, then $J = \frac{423 \cdot 55}{13 \cdot 596} = 31 \cdot 153$; also $l_0 = 80$, $t_0 = 273^\circ \cdot 7$, $v_0 = 209 \cdot 9$ (calculated by theory) ; whence $\frac{Jl_0}{v_0 t_0} = 0 \cdot 043$.   M. Kirchhoff remarks :—" This difference is too small to be safely inferred from Regnault's experiments.   It is interesting, however, to remark that a difference of the same sign and order of magnitude as the theory requires is furnished by the numbers which Regnault gives as the results of his experiments."   For M. Regnault's empirical formula for the pressure of steam over ice gives $\varpi'_0 = 0 \cdot 361$, and that for the pressure of steam over water gives $\varpi_0 = 0 \cdot 329$; whence $\varpi'_0 - \varpi_0 = 0 \cdot 032$. The lines therefore cut each other ; and the angle of intersection is re-entrant downwards as $\varpi_0 < \varpi_0'$.   M. Kirchhoff's numbers do not agree so well as the above, since he assumed that both the above empirical formulæ give the same value for p at 0° C., whereas the one gives $4 \cdot 610$ and the other $4 \cdot 600$.

M. Moutier has lately taken up the same line of reasoning, in a paper, " Recherches sur les vapeurs émises à la même température par un même corps sous deux états differents," published in *Annales de Chimie et de Physique*, [5] i. (1874) p. 343.   He has, however, assumed that $\varpi' = \varpi$; whence it would follow that at no temperature could water and ice have the same vapour-tension.   This mistake has been corrected in the 'Proceedings' of the Royal Society, 1874, xxii. p. 461, by Prof. Rücker, who reasons from Prof. James Thomson's conclusions, but without exactly reproducing M. Kirchhoff's result.

May 15th, 1877.

LXIX. *On a Modification of* Mance's *Method of measuring Battery Resistance.* By OLIVER J. LODGE, *B.Sc.**

[Plate V.]

THE modification here suggested consists simply in using a galvanometer and condenser instead of a galvanometer alone, so as to detect variations in difference of potential instead of variations in current.

By this change it is converted into a strictly *null* method. Moreover it is now possible entirely to get rid of the effects of variations in the electromotive force of the battery, which are very annoying in any of the ordinary methods and prevent accurate measure. This is accomplished by breaking the galvanometer-circuit the instant after the battery is short-circuited. Fig. 1 (Pl. V.) is a diagram of the connexions for measuring the resistance of the battery d, with the keys shown on a large scale : m partially short-circuits the battery when depressed ; n closes the galvanometer and condenser circuit *unless* depressed. The two keys are electrically independent ; but the stand of the upper one is balanced so as to rest partly on the spring of the lower one (which must be strong). On depressing the upper key, the first effect is to close the circuit marked r at the point m ; the second, and immediately succeeding, effect is to break the circuit marked g at the point n. The same object would be accomplished. more conveniently by a single double-contact key made on purpose, as shown in fig. 4. The object of the double key is fully explained below. A B C D represents a box of resistance-coils; a and b are large and equal resistances ; and c will be equal to d, the resistance of the battery, whenever the galvanometer-needle is unaffected by pressing down the keys.

Resistance-measurements in general.

Consider the arrangement of six conductors joining four points (commonly known as the Wheatstone's bridge) as forming the edges of a tetrahedron or triangular pyramid (fig. 2). It is obvious, (1) that, as far as position is concerned, every conductor has precisely the same properties as any other, and (2) that any one conductor is adjacent to four of the others and opposite to the remaining one. Call the resistances of pairs of opposite ones a and c, b and d, r and g, and let electromotive forces be caused to act in any manner through any of them ; then it can be shown that when $ac=bd$, r and g are " conjugate conductors," or that variations in the conductor r have no effect whatever on the current in g, and *vice versâ*, no

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* Communicated by the Physical Society.

matter whether these variations are simple changes of resistance or the introduction of new electromotive force.

By ascertaining, then, whether the insertion or removal of batteries at r has any effect on a galvanometer at g, one can observe whether the relation $ac = bd$ is or is not fulfilled, and can change one of these resistances until it is. For the case when a, b, c, and d are simple metallic conductors, this is Wheatstone's method of comparing their resistances.

Again, reciprocally, when this relation is fulfilled, no change in g can affect the current through the battery in r; and therefore, if this battery in r is the only electromotive force in action, a change in the resistance of g does not affect the current at all anywhere. Hence a galvanometer in, say, d will show a constant deflection whether the resistance of g is 0 or ∞, whenever $ac = bd$; and this is Thomson's method of measuring the resistance of the galvanometer d.

Further, from what has been said, there is no objection to an existence of electromotive force in any or all of the conductors, *provided it remains constant*; for it will be equally possible to observe whether changes in r (of any sort) have any effect on the current in g; and if not, then $ac = bd$, as before. For the case when d is a battery of constant electromotive force, this is Mance's method of determining its resistance.

But it must now be observed that although changes in r may have no effect on the current in g, they must affect very essentially the current every where else, and therefore through the battery d. This battery ought, then, to preserve its electromotive force constant in spite of variations taking place in the strength of its current—a thing which no known battery is capable of doing. The electromotive force of every battery is really a function of the current it is producing and of the time it has lasted. In cells called *constant* the dependence of electromotive force on current and time is only slight; but in none does it disappear.

This fact that the current [*] and consequently, to some extent, the electromotive force of the battery are made to vary by the process of measuring its resistance, constitutes a great apparent defect of the method; but it is an irremediable defect, and is not peculiar to this particular method. It is in fact *impossible* to measure the resistance of a battery without varying the strength of the current passing through it, by any method founded, as all our methods are, on a measurement of current or of difference of potential. In other words, just as it is im-

[*] Professor Clerk Maxwell, in describing this method ('Electricity and Magnetism,' i. p. 411), says that " the current in the battery is not in any way interfered with during the operation;" but this must be a mistake.

possible to measure any resistance whatever without the passage of a current through the resisting body, although it is quite easy to measure an electromotive force without any current circulating through the electromotor, so, although a current of constant strength is sufficient to give a measure of the resistance of a homogeneous conductor, such as a metallic wire of uniform material, or a homogeneous liquid, or anything else which contains no internal electromotive force, yet a variable strength of current is necessary to determine the resistance of an electromotor.

And the reason of this is apparent, viz. that the opposition experienced by a current in passing through an electromotor is of two kinds—one due to the proper *ohmic* resistance, the other due to the electromotive force ; and with only one strength of current it is no more possible to tell how much of the opposition is resistance and how much is electromotive force, than it is to obtain the values of two unknown quantities from one equation. We may either take two measures of the strength of current and then eliminate one of the unknown quantities algebraically, or we may use a contrivance (like Mance's method) by which one of them (viz. electromotive force) is eliminated electrically ; but two strengths of current are just as essential in the latter case as in the former, as also it is just as necessary that the two unknown quantities shall remain constant. It is possible that the resistance, as well as the electromotive force, of a battery does not accurately fulfil this condition, but that it varies to some extent with the current ; in so far as it does this, however, it is not a definite thing, and is incapable of accurate measurement.

I have entered into this matter at some length because the slip in Maxwell is getting repeated in other books (*cf.* Cumming's admirable ' Introduction to the Theory of Electricity,' p. 162) ; and it is as well to get clear on the subject.

The difference of potential required to force a current of strength C through an electromotor of resistance R and internal electromotive force ϵ is

$$E = RC \pm \epsilon.$$

Various methods may be applied to measure E and C ; but no observation of a single value of E and C can determine R unless ϵ is known. Another observation with a different value of E and C must be made ; and then ϵ can either be eliminated directly, or one can employ an indirect means of effecting its elimination, provided it remain constant. (If it does not remain constant, and if the law of its variation is unknown, no amount of experiments can eliminate it.) It is true that a

single strength of current will suffice to determine R after ϵ is known; but in the determination of ϵ another and quite different strength of current (viz. zero) was employed.

A curious illustration of the impossibility of measuring the resistance of an electromotor by means of a constant current was noticed the other day in the physical laboratory at University College by Mr. H. F. Morley, who has found that the current produced by a certain form of gas-battery is, within very wide limits, almost independent of the resistance of its circuit. He endeavoured to measure the internal resistance of this battery by means of its own current, but found it quite impracticable.

Variation of the Electromotive Force of a Battery.

In what precise way the electromotive force of an ordinary cell depends on the current passing through it and on the time that current has lasted, I am not aware of any experiments which afford us information. But a law like the following seems not improbable.

$$E = Ae^{-\frac{pt}{R}} + B, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where t is the time the cell has been in action through the resistance R; so that the rate of change of E is proportional to the excess of the strength of current it is producing over the minimum strength to which it will ultimately fall, or

$$\frac{dE}{dt} = -p\frac{E-B}{R}: \quad . \quad . \quad . \quad . \quad (2)$$

p is a number which may be constant, or it may be a function of the current or of t; but for a cell making any pretensions to constancy, it must be small. A and B are constants such that $A + B$ is the initial and B, the final, value of E.

At any rate we may, I think, reasonably assume that E is not affected instantaneously, however much the resistance of the circuit R is changed, but that it takes a certain time to change its value appreciably; consequently, if we only change R for an instant of time and then restore it to its original value, E may be regarded as constant. It is this fact, I apprehend, which gives Mance's method its practical value, and renders it superior to the somewhat similar methods of Siemens and Thomson, because in it the change of resistance of r can be made very rapidly without disturbing the galvanometer, and need only last a few seconds. The shortest time, however, is sufficient for some variation to take place in the battery; and accordingly a kick of the needle is usually

observed, like that produced by an extra current, which is very annoying. The modification which I have to propose, however, renders possible so great a virtual diminution of the period of contact that this disappears.

Modification of Mance's Method.

There is also a practical objection to the ordinary form of Mance's method, not relating to its essentials, but to its sensibility and convenience, which the modification is intended entirely to remove. It is this :—The galvanometer in g, whose function it is to indicate any change in the current in that branch, has always a certain current passing through it, and its needle is therefore deflected more or less, according to the sensibility of the galvanometer ; but the current produced by an ordinary cell whose resistance one wishes to measure is usually such as one does not care to pass through a delicate instrument, even if the excessive deviation it produces be corrected by external magnets. A rough galvanometer is therefore generally employed, and the needle is brought back reasonably near its mean position by magnets placed near it. But the needle being thus constrained by immersion in a powerful magnetic field, is by no means under favourable conditions, and only comparatively large changes in the current can be indicated by it. To remedy this defect and to make the method a *null* one, my first idea was to use a differential galvanometer and to send through its second wire a current from an auxiliary battery equal and opposite to the current produced in its first wire by the cell whose resistance is being determined, so as really to neutralize instead of merely to overpower its action on the needle. Or, without using a differential galvanometer, we may balance the electromotive force in the galvanometer circuit by means of an auxiliary closed battery circuit after the manner of Poggendorff. If either of these arrangements be adopted, we can use a sensitive Thomson's galvanometer, and its needle may be as nearly astatic as we choose. But it is not easy to get the two batteries under such similar conditions that they shall constantly oppose one another exactly ; and though these arrangements may be useful in some cases, they are rather complicated and the adjustments somewhat difficult to make.

The next alteration which suggested itself consisted in interposing a condenser in the galvanometer circuit (see fig. 3). This effectually prevents any continuous circulation of electricity in that branch ; and the galvanometer therefore remains at zero after the condenser has acquired its full charge ; but any variation in this charge is indicated by a throw of the

galvanometer-needle proportional to the amount of variation. The quantity of electricity flowing into or out of the condenser through the galvanometer-coil will be equal to the variation of potential, Y, taking place between its terminals multiplied by S, its statical capacity ; and the throw of the galvanometer-needle α will be proportional to this quantity multiplied by the galvanometer-constant Γ, which depends directly on the number of turns of wire on it.   The resistance of the galvanometer is quite immaterial.   If H is the strength of the magnetic field in which the needle hangs and T the time of a complete oscillation of the needle in that field, we have

$$\sin\frac{\alpha}{2} = \frac{\pi\Gamma SY}{HT}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

By using, therefore, a galvanometer with a very large number of turns, and a condenser of great capacity, one can increase the sensitiveness of the method to any extent.

The investigation of the distribution of currents throughout the circuit becomes very simple now that there is no continuous current through the branch g.   The connexions are shown in fig. 3, where AC is the branch r, whose resistance can be changed at pleasure from infinity to something near zero.   Let A, B, C, D, be the potentials of the four corners ; let d be the resistance of the battery we wish to measure, ϵ its electromotive force, and u the strength of the current passing through it.   We want the difference of potential $B-D$ to be wholly independent of the potentials of A and C, which will be altered by changing r.   Now as there is no current through g, we have the same current passing through b as through a—that is,

$$\frac{A-B}{a} = \frac{B-C}{b}, \text{ or } B = \frac{Ab+Ca}{a+b} ;$$

similarly

$$D = \frac{(A-\epsilon)c+Cd}{c+d} ;$$

hence

$$B-D = \frac{(A-C)(bd-ac)+\epsilon c(a+b)}{(a+b)(c+d)}, \quad . \quad . \quad (4)$$

which shows that the difference of potentials between the terminals of the condenser is independent of the potentials A and C as soon as the condition $bd-ac=0$ is satisfied.

We may conveniently write the above expression in terms of the strength of the current u passing through the battery

d ; thus, since $\mathrm{A-C} = \epsilon - (c+d)u$,

$$\mathrm{B-D} = \frac{(ac-bd)u + \epsilon b}{a+b}. \quad . \quad . \quad . \quad (4\,a)$$

So, if $ac = bd$, the difference of potential $\mathrm{B-D}$ is quite independent of the current through the cell (except in so far as the electromotive force ϵ depends upon it) and is equal to $\dfrac{\epsilon b}{a+b}$ or $\dfrac{\epsilon c}{c+d}$, which are the same thing.

The current u is of course dependent on the resistance r of the branch AC, being

$$u = \frac{(a+b+r)\epsilon}{(a+b)(c+d) + r(a+b+c+d)}; \quad . \quad . \quad (5)$$

so we may also write the above difference of potential in terms of this resistance r, thus :—

$$\mathrm{B-D} = \epsilon \cdot \frac{(a+b)c + r(b+c)}{(a+b)(c+d) + r(a+b+c+d)}. \quad . \quad . \quad (4\,b)$$

All the differential coefficients of this with respect to r contain the factor $ac-bd$; consequently when this factor vanishes this quantity is independent of r.

Conditions for Sensitiveness.

To find out what are the values of a, b, and c which give the greatest sensitiveness, we can subtract the value of $\mathrm{B-D}$ when r is infinite from its value when r is zero, and can make this quantity a maximum when the condition $ac = bd$ is nearly fulfilled. The quantity which has to be a maximum is

$$y = (\mathrm{B-D})_0 - (\mathrm{B-D})_\infty = \frac{\epsilon(ac-bd)}{(c+d)(a+b+c+d)}. \quad . \quad . \quad (6)$$

The resistance d is supposed to be given ; so let us define the others with reference to it, putting

$$c = \lambda d, \quad a = \mu d, \text{ and } \quad b \eqsim \lambda\mu d = \lambda\mu(1-z)d,$$

where z is a small quantity ; then the above quantity becomes

$$y = \frac{\lambda\mu z\epsilon}{(\lambda+1)^2(\mu+1)}. \quad . \quad . \quad . \quad . \quad (6\,a)$$

Considered as a function of λ, this is a maximum when $\lambda = 1$; it has in fact the same value whether $\lambda = n$ or $\dfrac{1}{n}$. Considered as a function of μ, it has no maximum, but it is greatest when μ is infinite, though it does not increase fast after μ is tolerably large ; the curve is, in fact, a rectangular hyperbola with asymptotes $y = 1$ and $\mu = -1$; and 1 is its greatest value for positive values of μ. Accordingly the most sensitive arrange-

ment is obtained when $\lambda = 1$ and when $\mu = \infty$ —that is to say, when c is equal to d (the resistance to be measured), and when a and b are equal and as large as convenient.   When these arrangements are made, the maximum value of y, or the change in the difference of potential between the terminals of the condenser brought about by depressing the key, is, when c is nearly equal to d,

$$Y = \tfrac{1}{4}\epsilon z \quad \text{or} \ = \frac{\epsilon}{4} \cdot \frac{c-d}{d} \,; \quad . \quad . \quad . \quad . \quad (7)$$

and this is the quantity to be inserted in equation (3).

The sensitiveness is evidently directly proportional to the electromotive force of the cell : but it is independent of its resistance; *i. e.* a high resistance is measured with as great proportional accuracy as a low one.   But it must be remembered that when the resistance to be measured is great, the resistances a and b should be as great also ; if they are not as great as d, the sensitiveness falls off very appreciably.   But, as said before, there is really no limit to the sensitiveness of the method; for the size of the condenser and the length of wire on the galvanometer may be increased to any extent.

Practical Details.

The condenser I have used is a small standard one with the dielectric of mica; and it has a capacity of slightly over one micro-farad.   The galvanometer is a Thomson astatic by Elliott, with a resistance of about 7000 ohms.   The two branches a and b of the resistance-coils, forming the equal arms of the bridge, were 1000 ohms each, being the largest resistance conveniently available in the box of resistance-coils used.   But when the resistance to be measured is large (say over 500 ohms) it is better to have a and b greater than this ; and they may then be made of Muirhead's carbon-paper (fig. 4).   A strip 2 feet long by half an inch broad will have a resistance of about 14000 ohms ; and the galvanometer terminal B may be connected with its middle so as to divide it into two halves representing a and b.   Exact equality in the two arms is not essential, as it is easy (and, indeed, generally advisable) to eliminate any errors of this sort by a method analogous to double weighing. Connect a and b to a commutator in such a way that it is easy to interchange them end for end (see fig. 4), and balance the resistance d by the coils c ; then interchange a and b and balance again; this time we may require an amount c'.   Then it is easy to see that $d = \sqrt{(cc')}$, whatever the ratio of a to b ; for in the first case we have $d : c = a : b$, and in the second $d : c' = b : a$.   If c and c' are nearly equal, their arithmetic

mean may be taken instead of their geometric, as being easier
to calculate.

Use of a Double-wire Galvanometer as an Electrometer.

When a differential galvanometer, or a galvanometer with
two long fine wires wound side by side, is employed, a sepa-
rate condenser is not absolutely necessary; for the galvano-
meter itself has a certain capacity, and in order to charge one
of its wires up to the potential B, and the other down to the
potential D, a certain quantity of electricity must flow into
the one wire and out of the other, and any variation in this
quantity will affect the needle (though the galvanometer-con-
stant has only half its ordinary value). Even when a separate
condenser is used the capacity of the differential galvanometer
may be taken advantage of, by connecting the terminals of the
condenser to its two middle screws (instead of joining them di-
rectly to each other by a wire and inserting the condenser as in
fig. 3), so that both condenser and galvanometer get charged
instead of only the condenser. The defect of this method is,
that the insulation between two silk-covered wires is not very
perfect, and there is a slight leakage, which maintains a slight
continuous deflection of the needle when the two outer screws
are joined up to a battery; moreover the statical capa-
city of an ordinary fine-wire differential galvanometer is not
very great.

But I think it may be often convenient to use a double-wire
galvanometer as an electromoter in this way. For instance,
rapidly to compare the electromotive force of any number of
cells, join them up to the outer screws of the galvanometer
with disconnected wires one after the other; the kick in each
case measures the electromotive force of the cell. It might
also be used to measure very high resistances. It is quite
possible, and indeed very probable, that this method has been
suggested before.

Elimination of Variations in Electromotive Force.

It has been stated above that if only momentary variations
are made in the resistance r, or in the value of u, we can con-
sider ϵ, the electromotive force of the battery, constant. The
plan I adopt is to make the *effective* variation of r, or the va-
riation which is to have any influence on the galvanometer,
very short indeed. And this is done by arranging that the
key m which closes the circuit of r shall break the galvano-
meter circuit g, the instant after, at the point n, as shown in
figs. 1 and 4. For an instant, then, u is varied; and if the
resistances are not balanced so that $ac=bd$, a certain quantity
of electricity will enter or leave the condenser through the

galvanometer ; but variations in ϵ (due to the changed u), which would produce the same effect on the galvanometer, no matter how much the resistances were balanced, have no time to take place before the galvanometer circuit is broken ; and then no further change has any effect.   This works very well in practice ; and the resistance of a cell can be really determined when producing a current through a definite resistance, viz. $a + b + c + d$.   This cannot be done accurately by any other method that I know of.

Measurement of any Liquid Resistances.

The method may be applied to determine the resistance of electrolytes in general.   A long tube containing the electrolyte surrounded by a jacket of water at a known temperature is interposed in the battery circuit d, the battery being one whose resistance is small and can be depended on ; and the resistance of the two together, battery and tube, is measured.

The tube is then removed, and the resistance of the battery determined alone ; the difference of course gives the resistance of the electrolyte in the tube.   The tube can then be filled with mercury and the measurement repeated.   The amount of polarization of the electrodes is of no more consequence than the electromotive force of the battery, provided the gas given off is not allowed mechanically to obstruct the current ; and the effect of variations in its amount are reduced to a minimum by the method just described for the battery.   It is well to make the tube end in a couple of globular receivers with two necks, very much like Dewar's electrometer, and to plunge large electrodes into these globes (see T, fig. 4).   Their position in the globes is not of very much consequence ; neither is a bubble or two of gas on their surface ; the principal part of the resistance is offered by the liquid in the tube.   If a thermometer is kept in each receiver and the liquid be allowed to flow backwards and forwards through the tube once or twice before observing, its temperature may be known without surrounding it with a jacket.   But of course this does not apply accurately when it is required to raise its temperature much above that of the atmosphere.   Since the sensitiveness depends on the electromotive force of the battery, it is well to use one or two Grove's cells.   Fig. 4 is a complete diagram of the connexions, showing the arrangement for interchanging a and b by means of a commutator, and also showing a key which will break the galvanometer circuit at the instant required and act instead of the extemporized arrangement of two keys depicted in fig. 1.   The lower spring carries a block which presses up against a screw connected with the galvanometer, except when it is forced down by the upper spring.   This

block is insulated from the spring, which carries it, but is connected with the point B. The interval elapsing between the breaking contact at m and the making at n is, and must be, utterly inappreciable. For the shortest practicable interval is sufficient to allow the currents to adjust themselves, unless a and b are extravagantly large resistances ; and if it is not exceedingly short, disturbances will occur due to variations in the battery.

The diagrams purposely show the galvanometer, and not the condenser, connected with the breaking-key n, because leakage of the condenser is sure to occur to some extent when its circuit is broken, and, in order that this may not in the least signify, one terminal of the galvanometer must be insulated.

University College, London.

LXX. *Additional Note on Ludlamite.*
By N. Story Maskelyne, *F.R.S.*[*]

THE column of calculated angles incorporated in my notice of Ludlamite is vitiated by an error traceable to the erroneous reading of a figure in the calculation of one of the fundamental angles ; and a consequent revision, at somewhat greater leisure, of the relative weights to be attached to the measurements of the different angles on the various crystals, has enabled me to offer a closer approximation to the true elements of Ludlamite than I was able to give at the time the measurements were published. The subjoined data therefore present a better theoretical representation of the crystallography of Ludlamite than that published in the Philosophical Magazine of January last.

System oblique, the elements of the crystal being

$$1\,0\,1\,.\,1\,0\,0=42^\circ\ 46',\quad 1\,1\,1\,.\,0\,1\,0=36^\circ\ 37',$$
$$1\,0\,1\,.\,0\,0\,1=36^\circ\ 41';$$

otherwise, $a : b : c = 1 : 0{\cdot}4439 : 0{\cdot}8798.$

$\eta = 100^\circ\ 33'$; *i. e.* the axial angle Z X $= 79^\circ\ 27'.$

$a\,c$	$=79\ 27$	$a\,m$	$=65\ 41\frac{1}{2}$	$c\,m$	$=85\ 40\frac{2}{3}$
$c\,d$	$=45\ 53$	$m\,m'$	$=48\ 37$	$c\,q$	$=68\ 31$
$d\,a'$	$=54\ 40$			$c\,p$	$=61\ 25\frac{1}{2}$
$a\,t$	$=26\ 49\frac{1}{2}$			$c\,r$	$=44\ 36\frac{1}{2}$
$k\,a'$	$=31\ 56\frac{1}{2}$				

$a\,p$	$=64\ \ 1$	$q\,q'$	$=63\ 28\frac{1}{2}$	$k\,q$	$=\ 60\ 58\frac{1}{2}$
$a\,l$	$=85\ 12\frac{2}{3}$	$d\,q$	$=58\ 15\frac{3}{4}$	$k\,m$	$=110\ 27$
$l\,q$	$=22\ 30\frac{2}{3}$	$p\,n$	$=53\ 23$	$c\,l$	$=\ 62\ 50$
$q\,a'$	$=72\ 17\frac{1}{3}$				

[*] Communicated by the Author.

LXXI. *Notices respecting New Books.*

*The Amateur Mechanic's Practical Handbook, describing the different
Tools required in the Workshop, the Uses of them, and How to Use
them; also Examples of different kinds of Work, with full De-
scriptions and Drawings. By* ARTHUR H. G. HOBSON. London:
Longmans, Green, and Co. 1877. (Small 8vo, pp. 114.)

THIS book contains a number of instructions for performing
different kinds of work, particularly metal work, such as an
amateur mechanic is likely to undertake. The principal tools
which he is supposed to have are a lathe, a drilling and planing
machine, as well as vice, bench, and hand tools. He is expected
to be able to use them: *e. g.* he should be able to turn a crank-
shaft, bore out a cylinder, cut a screw, and make a universal chuck.
He should also be able to make drawings and patterns. Although
it is scarcely worth the trouble, yet, at a pinch, our amateur ought
to be able to make his own iron castings. The case is different
with brass castings; by making them he will save both time and
money. He is supposed to be well off for space; for he should
have a separate room for pattern-making. When thus furnished
he will be prepared to execute works of considerable difficulty,
such as to make a horizontal engine; and accordingly a chapter is
devoted to describing the process. The boiler, however, is a more
difficult matter; so this may be "selected." Still, "if you are at a
loss for a job, you will find it some amusement to make a boiler
yourself;" and therefore instructions are given for making a
vertical boiler. For though "horizontal boilers may be used," yet
when the size is small "they do not answer so well as the upright
ones." When all is made, the amateur will have his reward; "he
lights the fire in his boiler, turns the steam on, and excitedly looks
for the first turn of the fly-wheel. If the engine be a success, he
will in that one turn of the fly-wheel pass one of the pleasantest
moments of his life."

It will be seen that our author is enthusiastic about his pursuit;
still he gives his amateur some very good and sensible advice: thus,
he tells him how to profit by a visit to a mechanical engineer's works,
and, again, advises him to learn not only how to mend his tools,
but even for the most part to make them. This advice, if followed,
would in many cases save his readers a good deal of money, by
enabling them to find out whether they have in them the genuine
spirit of the amateur mechanic: "Buy but few tools at first, you
will soon find out your wants as you go on. Make as many as
you can; and with diligence in using them, and exercising care and
patience, in the course of twelve months you will find yourself
amply repaid for your trouble." Very true! If you are not pre-
pared to make and mend your own tools, you have not the root of
the matter in you, and had better give it up.

Memoirs of the Geological Survey. Explanation of Map 48 E.S.E
and adjoining part of 48 N.E. *The Geology of the Eastern End
of Essex* (*Walton-Naze and Harwich*). By W. WHITAKER, *B.A.
Lond., F.G.S.* London, 1877. (8vo, pp. 32.)

This short memoir completes the geological descriptions, by the
Geological Survey, of the area directly bordering the estuary of the
Thames, and is supplementary to the author's longer memoir on
the Geology of the London Basin, 1872. It is also the first
memoir issued by the Geological Survey which notices the Crag
Formation, so rich in fossils, and valuable as a source of the phos-
phatic nodules and fossils, called " coprolites," used in making
artificial manures.

The London Clay and its cement-stones come to be first de-
scribed ; then the Red Crag of Beaumont, Walton-Naze, and near
Wrabness and Harwich, is carefully noticed, in brief, both from
the author's recent observation on its dwindling remnants, and
from the results of the long-continued researches of the Messrs.
Wood, Prof. Prestwich, and others. The old gravels, sands, and
brick-earths, and the later alluvium, blown sand, and shingle, also
receive attention. In noting the physical features of this clay
district, with its local cappings of gravel, and its alluvial flats, the
great waste of the seacliffs is of course mentioned. We may sug-
gest that if the Government were to apply a small sum of money
to allow of the Coast Guard making accurate periodical measure-
ments, from the sea to inland marks, on those parts of the British
coasts subject to great degradation, some definite bases for calcu-
lating the continuous loss of land, and occasional gain from the
sea, would be obtained before many years have elapsed.

Several valuable well-sections are recorded by Mr. Whitaker in
Appendix I. ; and Mr. Etheridge gives some carefully revised lists
of fossils in Appendix II.

LXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 477.]

Dec. 14, 1876.—Dr. J. Dalton Hooker, C.B., President, in the
Chair.

THE following paper was read :—
" Note on the Photographic Spectra of Stars." By William
Huggins, D.C.L., LL.D., F.R.S.

In the year 1863 Dr. Miller and myself obtained the photograph
of the spectrum of Sirius.

" On the 27th January, 1863, and on the 3rd March of the same
year, when the spectrum of this star (Sirius) was caused to fall
upon a sensitive collodion surface, an intense spectrum of the more
refrangible part was obtained. From want of accurate adjustment
of the focus, or from the motion of the star not being exactly

compensated by the clock movement, or from atmospheric tremor, the spectrum, though tolerably defined at the edges, presented no indications of lines.   Our other investigations have hitherto prevented us from continuing these experiments further; but we have not abandoned our intention of pursuing them "*.

I have recently resumed these experiments by the aid of the 18-inch speculum belonging to the Royal Society's telescope in my possession.   Considerable delay has arisen from the necessity, for these observations, of a more uniform motion of the driving-clock.   For this purpose, Mr. Howard Grubb has successfully applied to the clock the control of a seconds' pendulum in electric connexion with a sidereal clock.   This system works quite satisfactorily.

The prisms employed are made of Iceland spar, and the lenses of quartz.   After an extensive trial of different photographic processes, preference has been given to dry plates.

The apparatus is so arranged that a solar or electric spectrum can be taken on the same plate, for the purpose of comparison, with the spectrum of the star.   Spectra have been obtained of Sirius, Vega, Venus, the Moon, &c.

I do not purpose in this preliminary notice to describe in detail the arrangements of the special apparatus which has been constructed, nor to offer the results of the experiments in their present incomplete state to the Royal Society.   Still I venture to hope that, even in this early stage of the inquiry, the enlarged copy of the spectrum of Vega (α Lyræ) which accompanies this note may not
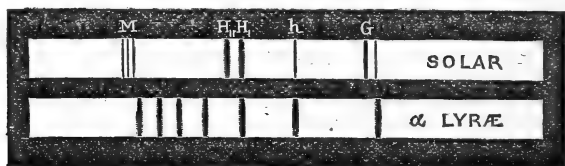
be regarded as altogether unworthy of attention.

After exposure to the light of Vega, the dry plate was allowed to remain in the instrument until the following morning, when a solar spectrum was taken upon it through the half of the slit which had remained closed when the instrument was directed to the star.

The photograph shows seven strong lines, all of them slightly shaded at the sides.   The two lines which are least refrangible coincide with two known lines of hydrogen in the solar spectrum.

It is expected, by means of an apparatus now in the course of construction, to obtain also any finer lines which may be present in the spectrum of this star, as well as to extend the photographic method to stars which are less bright.

I need not now refer to the many important questions in connexion with which photographic observations of stars may be of value.

* Phil. Trans. 1864, p. 428.

December 21.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

The following papers were read:—

" On the Rotation of the Plane of Polarization of Light by Reflection from the Pole of a Magnet." By George Francis Fitzgerald, M.A.

At a meeting of the Dublin Scientific Club on Monday the 6th November, Professor Barrett gave the Club an account of Mr. Kerr's experiments on the rotation of the plane of polarization of a ray of light when reflected from the surface of the end of a magnet, to which additional interest was attached by the reading of a letter from Mr. Kerr to Professor Barrett giving an account of the mode of making and of the last results of his experiments. At the time I proposed trying whether any similar effects would be produced by reflection from the surface of a crystal of quartz cut perpendicularly to the axis, as I was led to think there might be, owing to the similarity of the rotatory polarization of quartz and of substances under magnetic action. Following out that clue, I obtained the following explanation of Mr. Kerr's experiment, and was enabled, through Professor Barrett's kindness in helping me to verify my recollections of Mr. Kerr's letter, to make sure that my theory explains the facts.

Faraday has shown, in the nineteenth series of his experimental researches, that a ray of plane-polarized light, when transmitted through any solid (diamagnetic?) transparent medium under the action of a powerful magnet, has the plane of its polarization rotated in that direction in which a positive current must circulate round the ray in order to produce a magnetic force in the same direction as that which actually exists in the medium. Verdet, however, discovered that in certain *ferro-magnetic* media (as, for instance, a strong solution of perchloride of iron in wood-spirit or ether) the rotation is in the opposite direction to the current which would produce the magnetic force.

Now Fresnel's explanation of the rotatory power of quartz has been applied by Professor Maxwell, in his ' Electricity and Magnetism,' vol. ii. p. 402, to explain the similar, though not identical, phenomenon of magnetic rotation of light. He there, in § 812, gives this explanation in the following words:—" A plane-polarized ray falls on the medium. This is equivalent to two circularly polarized rays, one right- and the other left-handed (as regards the observer). After passing through the medium the ray is still plane-polarized, but the plane of its polarization is turned, say, to the right (as regards the observer). Hence of the two circularly polarized rays, that which is right-handed must have had its phase accelerated with respect to the other during its passage through the medium. In other words, the right-handed ray has performed a greater number of vibrations, and therefore has a smaller wavelength within the medium than the left-handed ray which has the same periodic time." This is the same as saying that the velocity

of the right-handed ray is less within the medium than the left-handed, or that the refractive index for right-handed rays is greater than for left-handed in a medium that rotates light to the right. Hence, from what Verdet has shown, it appears that, in a ferro-magnetic substance, *for a ray of light travelling from the south to the north pole, the magnetic action is such as to make the refractive index for right-handed circularly polarized rays less than for left-handed ones;* for in this case the plane of polarization is turned to the left, for it is a right-handed current that would produce the magnetic force.

By applying this to the case of light reflected from the south pole of a magnet, we get what I believe to be the true explanation of Mr. Kerr's interesting experiment. In like manner, as in the case of a transmitted ray, I consider the incident plane-polarized ray to be the resultant of two circularly polarized ones, one right- and the other left-handed. Now, for the right-handed one, the refractive index at the surface of the south pole of the magnet, being a ferro-magnetic substance, is less than for the left-handed ray. Hence if each of the two circularly polarized rays be supposed to be the resultant of two plane-polarized rays, one polarized in the plane of incidence and the other at right angles to it, the intensities of these four rays being equal, it is evident that the intensities of the pair of reflected rays corresponding to the left-handed ray will be greater than the corresponding intensities of those due to the right-handed ray. Hence the two rays which were polarized perpendicularly to the plane of incidence, and which originally destroyed one another, will, after reflection, have a component in the direction of the vibration of the left-handed ray after reflection. Now, on account of the change of direction of the ray on reflection, this latter is towards the right. This is completely explained in M. Jamin's 'Cours de Physique,' vol. iii. part 2, p. 674, where he shows that a ray the azimuth of whose plane of polarization was originally towards the right is by reflection turned towards the left. Hence the result of reflection is to furnish two rays, one polarized in the plane of incidence, and the other at right angles to it. The phases of these rays will, in general, be different; for they differed by 90° before reflection, and, except at the polarizing angle for iron, this difference of phase would not be completely destroyed, so that the resultant would generally be an elliptically polarized ray the direction of whose major axis would make a small angle towards the right with the plane of incidence; and at the polarizing angle for iron this ellipse would become a plane-polarized ray whose plane of polarization was turned towards the right, which I understand to be the direction in which Mr. Kerr observed it to be turned—although from some ambiguity as to the meaning of right and left rotations in a ray, arising from not specifying whether it is relative to the direction in which the ray is going or in which it is observed, I am not quite sure whether I understand Mr. Kerr correctly.

Also from the fact that there are exceptions * to the rule that rotations are positive for diamagnetic and negative for ferro-magnetic substances, neutral chromate of potash being diamagnetic, yet producing a negative rotation, I should be rather inclined to deduce the direction of the rotation that would be produced, if iron were transparent, from Mr. Kerr's experiment.

It would be quite easy to deduce the difference of the refractive indices of iron for the two circularly polarized rays if we knew the amount by which the plane of polarization is turned; but it would be necessary to employ MacCullagh's or Cauchy's formulæ for the intensities of the reflected rays; and these are so complicated that it is hardly worth while going through the calculations, as the effect Mr. Kerr has observed seems only barely observable.

Similar effects must, of course, occur in the cases of diamagnetic substances, organic solutions, and quartz; but the amounts in these cases would be entirely beyond the range of observation of our present instruments; for in quartz, for instance, the difference of the refractive indices of the two circularly polarized rays is only 0·00008.

Observations confirmatory of the foregoing Explanation.

Since sending my explanation of Mr. Kerr's experiment I have made some experiments in confirmation of it. The instruments, with the exception of the electro-magnet, which was kindly lent to me by Mr. Yeates, are the property of Trinity College, Dublin, and were placed at my disposal by Professor Leslie.

The electro-magnet I used is of the horseshoe pattern, with movable soft iron armatures, a face of one of these being well polished. The magnet was placed vertically, and the armatures were arranged on the poles so that the polished face was vertical and a vertical edge of the other armature parallel and very close to this face. A folded piece of paper was inserted at the top between the edge and the face to prevent their being drawn together when the magnet was set in action. Two Nicol's prisms were so placed that a horizontal beam of light traversing one of them was reflected down the other by the polished face from that part of its surface which was opposite the edge.

A beam of sunlight was now transmitted through the apparatus and observed on emerging from the second Nicol. The following results were thus obtained:—When the light was polarized by the first Nicol, either in or perpendicularly to the plane of incidence, and when it had been extinguished by the analyzer, as soon as the electro-magnet was set in action the light immediately reappeared. On now slightly moving the analyzer the light could be partly extinguished; but no motion of the analyzer could make the field as black as it had been before the magnetism was excited, thus conclusively proving that what was produced was an elliptically

* Unless, indeed, these are due to the nature of the solvent.

polarized ray, as I had anticipated. When the light was reflected from a south pole the plane of polarization was rotated to the right of the observer, which is the direction of rotation assumed in my explanation.

I next covered a portion of the polished face with gold leaf, as Professor Barrett had suggested; and now the light reflected from this diamagnetic substance was unaffected by the magnetism, as I had also anticipated. I exhibited all these effects to Mr. Stoney, who entirely confirmed my observations.

The angle of incidence in the experiments described above was about 60°. If the incidence were either perpendicular or grazing, the theory which I have proposed would lead to the conclusion that the angle between the major axis of elliptic polarization and the original plane of polarization would vanish. If, accordingly, the observation can be made at a perpendicular incidence, and if the Nicol's prisms be so placed as to extinguish the light before magnetizing the iron, then on exciting it light ought to reappear, as it does at oblique incidences; but the field should not become darker on moving the analyzer.

I attribute great weight to the verification of my theory arising from the fact that the polarization of the reflected ray is found by experiment to be in general elliptic, and also from the fact that there is no appreciable effect when gold, a diamagnetic substance and therefore feeble, is substituted for iron.

Since communicating my paper, I learn, through Professor Stokes, that when Mr. Kerr's paper was read before Section A of the British Association, both he and Sir W. Thomson spoke of the possibility of connecting Mr. Kerr's result with a powerful double refraction of the same kind as the feeble double refraction shown by transparent substances under the influence of magnetism. It is a connexion of this kind which I have endeavoured to demonstrate.

"On the Increase in Resistance to the Passage of an Electric Current produced on Wires by Stretching." By Herbert Tomlinson, B.A., Demonstrator of Natural Philosophy, King's College, London.

The object of this inquiry was

(1) To determine the relation between increased resistance to the passage of an electric current and stretching force.

(2) To ascertain how much of the increased resistance in each case is produced by mere increase of length and diminution of section of the stretched wire.

In order to determine the increase of resistance from stretching, the wires were each divided into two parts, about 14 ft. in length; one end of each part was fastened to a stout hook firmly fixed into a block of wood. These two hooks were about 8 inches apart, and the block of wood in which they were fixed was securely fastened across two uprights placed resting against a wall of the room, so that the weights, which were attached to the other ends

of the wires, might swing clear of the table. The two parts of the wire were joined at the top, about 2 inches below each hook, by a small piece of copper wire, which was securely soldered on to each part of the wire so as to connect them. Towards the lower extremities of the two parts, about 5 inches above the points of attachment of the weights, two copper wires of small resistance were soldered so as to connect the wires with a Wheatstone-bridge arrangement. The increase of resistance was measured by means of a sliding scale of platinum wire divided into millimetre divisions, each equal to ·00166 ohm. As the object was to obtain the temporary, and not the permanent, increase of resistance, which permanent increase was found more or less with all the wires, weights slightly heavier than those intended to be used were first put on and then taken off. Afterwards the wire was balanced as nearly as possible by German-silver wire without the sliding scale, and then very exactly with the sliding scale, which was connected with one of two resistance-coils of 100 ohms each, which formed the other two sides of the bridge. The weights used were then carefully put on to the wires, and the increase of resistance measured by means of the sliding scale; the weights were next taken off again, and the sliding scale used for balancing once more. If there was any slight difference, as sometimes occurred, between the readings of the sliding scale before the weights were put on and after they were taken off, the mean of the two readings was taken. In order to secure still greater accuracy, as many as eight or ten trials were frequently made with each particular weight, and the mean of all the trials taken. In this manner 4 pianoforte steel wires, 1 wire of commercial steel, 3 iron wires, and 4 brass wires were examined with several different weights. The wires taken were of various sections, and it was found that in each case the increase of resistance was " exactly proportional to the stretching force," the stretching not being carried beyond the limit of elasticity of each wire. The resistance of a cubic centimetre of each wire was then determined, also the increase of resistance which a cubic centimetre of each wire would experience when stretched by a force of 1 gramme in the same direction as the passage of the current was calculated from the observations made. The former values varied from

$1574\!\cdot\!8 \times 10^{-8}$ to $1882\!\cdot\!4 \times 10^{-8}$ in the case of steel, from

$1200\!\cdot\!8 \times 10^{-8}$ to $1291\!\cdot\!0 \times 10^{-8}$ in the case of iron, and from

$656\!\cdot\!7 \times 10^{-8}$ to $782\!\cdot\!2 \times 10^{-8}$ in the case of brass;

the latter values varied from

2982×10^{-17} to 3511×10^{-17} in the case of steel, from

2557×10^{-17} to 2712×10^{-17} in the case of iron, and from

1565×10^{-17} to 1843×10^{-17} in the case of brass,

the numbers in each case representing so many ohms.

On dividing the latter values by the former, it was found that the increase per unit of resistance for a stretching force of 1 gramme on a cubic centimetre of each wire was nearly the same for wires of the same material, but differed with wires of different materials. The mean increase per unit of resistance was

$$\text{for the steel wires } 1875{\cdot}5 \times 10^{-12},$$
$$\text{for the iron } \text{,, } 2132{\cdot}2 \times 10^{-12},$$
$$\text{and for the brass } \text{,, } 2244{\cdot}9 \times 10^{-12},$$

the greatest departure from the mean value being

for the steel less than 2·7 per cent.,
for the iron about 3·0 per cent.,
and for the brass about 8·5 per cent.

The temporary increase of length which a cubic centimetre of each wire would experience on being stretched with a force of 1 gramme was then calculated from observations which had been made in the usual manner with the cathetometer; this increase of length was found to vary

in the case of 3 steel wires from 5082×10^{-13} to 5665×10^{-13},

in the case of the iron wires from 4896×10^{-13} to 5938×10^{-13},

and in the case of 1 brass wire was 10120×10^{-13}.

On dividing the increase per unit of resistance for a stretching force of 1 gramme on a centimetre of the material by the increase of length produced by the stretching force, so as to obtain the increase per unit of resistance when the wires are stretched 1 centimetre, a mean value of 3·525 was obtained for the steel wires, 3·951 for the iron wires, and 2·203 for the brass wires—thus showing that, though the increase per unit of resistance for a given stretching force is greater in brass than in iron or steel, the increase per unit of resistance for a given lengthening of the wire is much greater both in iron and steel than in brass.

The torsional rigidity of the wires was next ascertained by the method of vibrations, several trials being made with different lengths of each wire; the results for different lengths of the same wire agreed very closely indeed.

From the values of torsional rigidity and the increase of length, the diminution of section was calculated for a cubic centimetre of each wire when stretched with a force of 1 gramme, assuming the wire to be isotropic. Next the increase of resistance which would result from mere lengthening of each wire and diminution of section was determined, and it was ascertained that, on subtracting this latter value from the total observed increase of resistance, there was a considerable residue in the case of the steel and iron wire, also a residue not so great in the brass. This residual increase of resistance probably arises from increased

space in the line of flow of the current between the particles of the wire produced by the stretching force.

The conclusions to be drawn from the experiments are :—

1. That the temporary increase per cent. of resistance of a wire when stretched in the same direction as the line of flow of the current is exactly proportional to the stretching force.

2. That the increase per cent. of resistance, when a cube of each material is stretched by the same weight, is greater in iron than in steel wire, and greater in brass than in iron ; also that the increase is nearly the same for different specimens of the same material.

3. That the increase per cent., when a cube of each material is stretched to the same extent, is much greater in iron and steel than in brass.

4. That there is a residual increase in each case over and above that which would follow from mere increase of length and diminution of section ; that this residual increase is much greater in iron and steel than in brass, and greater in iron than in steel.

February 15, 1877.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

The following paper was read :—

" On Stratified Discharges.—III. On a rapid Contact-breaker, and the Phenomena of the Flow." By William Spottiswoode, M.A., F.R.S.

In a paper published in the Proceedings of the Royal Society, vol. xxiii. p. 455, I have described a form of contact-breaker designed for great rapidity and steadiness of action. It consisted of a steel rod which vibrated under the action of an electromagnet. As regards sharpness of break and steadiness of definition in the striæ, this instrument left little or nothing to be desired. But, as explained in the paper above quoted, an alteration in the current not only affected the steadiness directly, but also reacted on the break itself. The effects due to an alteration of the current alone thereby became masked, and the study of the laws relating to such changes was rendered more difficult, or altogether impracticable. In order to obviate this inconvenience I devised another form of contact-breaker, in which the vibrating rod and electromagnet were replaced by an arrangement purely mechanical in its action, and therefore entirely under control.

This instrument consists essentially of a wheel platinized at the edge, on which a platinum spring rests. In the circumference of the wheel a number (40 in the first instance) of slots were cut, and filled with ebonite plugs so as to interrupt the current. The breadth of the slots was about ·04 inch, and that of the teeth about ·5 inch. The wheel was connected with suitable driving gear, so as to give from 250 to 2000 currents from the coil in each direction per second. A 4-inch coil was found sufficient to produce the effects ; but the 18-inch coil by Apps, mentioned in former communications, was preferable. With the wheel, as with the electromagnetic break, a very slight strength of current was

required ; but, on the other hand, high tension in the primary was found necessary. In many of the experiments accordingly from 10 to 20 of the smallest Leclanché cells usually made were employed with the small, and from 20 to 50 with the large coil.

But these were afterwards replaced by a double fluid battery suggested by my assistant, Mr. P. Ward, and described at the end of this communication.

For some time the experiments were conducted with the platinum spring resting on the wheel; and the effects were varied by altering either the pressure of the spring or the velocity of the wheel ; but the gradual abrasion of the platinum through friction proved to be a fruitful source of irregular results. This irregularity of action, at all times difficult to compensate, and sometimes insuperable, was fortunately removed by a simple although delicate adjustment. It was, in fact, found that actual metallic contact between the spring and wheel was not necessary, provided that a layer or cushion of conducting material were interposed. Such a layer was formed by a thin film of liquid drawn out by a thread leading from a reservoir and resting on the wheel. Various fluids were tried ; but the simplest, and on the whole the best, proved to be dilute sulphuric acid, in the proportion of 1 drop of acid to 6 drams of water. Generally speaking the better conductor the fluid is, the better are the obtained results but, owing to the insulating slots being very narrow in this instance, a comparatively weak mixture of acid and water was necessary. In one wheel, where the insulating slots are $\frac{1}{4}$ in. wide, a mixture 36 times as strong may as advantageously be used. The spring, which under these circumstances became unnecessary, was replaced by a point, the adjustment of whose distance from the wheel was simpler and more accurate. This arrangement gave excellent results, even when the number of currents per second was reduced in some cases to 250 ; added to which the unpleasant and disturbing noise of the friction was entirely avoided.

Wheels having different numbers of teeth were also constructed, and (what was perhaps of more importance) having teeth of different breadths, so as to give with the same velocity of rotation contacts of different duration. The breadth of the ebonite plugs, or length of interruption of the current, was immaterial, so long as the current was efficiently broken. It did not appear, however, that with the same tube more could be obtained with wheels having different numbers of teeth, than with the same wheel at different speeds. But it was found that for different tubes different wheels occasionally gave better results.

With the contact-breaker here described effects similar to those produced by the rapidly vibrating break were obtained. The striæ were formed in a regular manner, and advanced or receded, or remained at rest, in a column usually unbroken, so long as the velocity of the wheel was maintained without change ; and even in the longer tubes, where the striæ, of the double discharge, advanced or receded towards both ends at the same time, and appeared some-

times compressed and at others dilated, the phenomena always maintained their characteristic features.

The condition of the striæ here described, whether flowing or stationary, may be comprised under the general term " steady;" and when there is no motion in either direction, they may be specifically denominated as " stationary."

Two questions here presented themselves :—First, what are the conditions necessary for the production of steady striæ ? Secondly, what are the conditions and circumstances of the advance and retreat, in other words, of the flow of steady striæ ?

With a view of ascertaining the nature of the distinction between the ordinary and the steady striæ, careful observations were made with the revolving mirror. It having been noticed that when the wheel break moved slowly ordinary or irregular striæ were produced, and that when it moved rapidly steady striæ resulted, it seemed probable that the latter effect might be due to the short time of contact, and to the consequent absence of many of the features described in Part II. of these researches. This is, in fact, identical with the suggestion there made, that the fluttering appearance was due to the unequal duration of the striæ themselves, and to the irregular positions of the points at which they were renewed at successive discharges of the coil. And such, in fact, proved to be the case ; for as the speed of the wheel was increased the duration of the discharges diminished ; the image as seen in the mirror became narrower and simpler in its configuration, until, when the steady effect was produced, each discharge showed only a single column of striæ of a width proportional to the apparent width of the slit. The proper motion, implied by the inclination of the individual striæ to the vertical, was still perceptible, and was directed, as usual, towards the negative pole.

From a comparison of the number of striæ as seen by the eye with those seen in the revolving mirror, it was found that the striæ so formed were of the kind called " simple " in former communications. And the phenomena of the flow may therefore be considered to be due to the different positions taken up by the striæ in successive discharges. If in each discharge the striæ occupy positions in advance of those occupied in a previous discharge, the column will appear to advance ; if the reverse be the case, they will appear to recede. If the positions remain unchanged, the column will appear stationary.

The following consequence of this explanation of the flow will readily occur to the reader, viz. that the rapidity of the flow will increase with the extent of advance made by the striæ in each successive discharge, until that advance amounts to half the distance between two contiguous striæ. Before this is attained the flow will have become too rapid to be followed by the unassisted eye, and can only be observed by the aid of the mirror. When this rate of advance has been exceeded, the flow will appear to be reversed. If the rate of advance still continues to increase, the rapidity in the reverse direction will appear to decrease until the advance amounts

to the entire distance between two contiguous striæ, when it will apparently be reduced to zero; the striæ will then again appear stationary. Experiments appear to confirm this view of the case.

Experiments were next instituted with a view of ascertaining the connexion between the flow and resistance. Starting from a condition of current and break for which the striæ were stationary, it was found that an increase of resistance, introduced generally in the primary circuit, produced a forward flow, *i. e.* from the positive towards the negative terminal, while under similar circumstances a decrease of resistance produced a backward flow. Furthermore, if after producing a forward flow the resistance be continually increased, the flow after increasing in rapidity so as to become indistinguishable by the unassisted eye, gradually appears to become slower, and ultimately to reverse itself, in accordance with the law suggested above.

Another form of contact-breaker was also occasionally used. The principle upon which it was based was the sudden disruption of a thin film of conducting liquid by a discharge between the electrodes of a circuit. The mode of effecting this was to make one electrode terminate in a platinum plate fixed in a horizontal position, and supplied with a uniform film of dilute sulphuric acid; the other in a platinum point, the distance of which from the plate is capable of delicate adjustment by means of a screw. Electromotive force required for this break is not less than that of 5 cells of Grove.

As soon as the current passes, the fluid between the plate and point will be decomposed and electrical continuity broken. This done, the fluid flows back again, and continuity is restored. By a proper adjustment of the supply of fluid and of the distance of the electrodes (the latter varying from ·05 inch to ·001 inch), the number of disruptions may be made to attain 1000 per second.

The currents delivered by this form of break are exceedingly uniform, and the effects produced are quite equal in delicacy to those produced by the electromagnetic or by the wheel break.

The elements used in the battery to which allusion was made in the early part of this paper are zinc and carbon. The zinc is immersed in dilute sulphuric acid in the proportions of 1 volume of acid to 7 of water; and the carbon in a saturated solution of bichromate of potash with 1 volume of sulphuric acid to 7 of the solution. The carbon and bichromate solution are held in a porous cell.

The absence of nitric acid permits this battery to be used in a room; while the fact that the zinc is attacked only when the circuit is complete, renders it unnecessary to lift the plates out of the fluid when not in use, as in the bichromate battery. The only limit to the time during which this battery may be left untouched, appears to be the period when the bichromate salt finds its way into the outer cell, so as to attack the zinc independently of electrical action. But this does not take place to an extent materially to affect the action for some months.

[Continued from p. 395.]
April 25th, 1877.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

The following communications were read :—

1. "On the Upper Limit of the essentially Marine Beds of the
Carboniferous System, and the necessity for the establishment of a
'Middle Carboniferous Group.'" By Prof. E. Hull, F.R.S., F.G.S.

The author, in this paper, divided the whole of the Carboniferous
rocks into successive stages from A to G inclusive, taking the Car-
boniferous beds of Lancashire as a type, and showed that these
stages could be identified over the whole of the British Isles. It
was only recently that their determination had been made in Ire-
land, so that until now the materials had not existed for a complete
correlation of the series in the British Islands. The following is an
abbreviated statement of the representative stages in descending
order :—

Essentially Freshwater or Estuarine, with one or two Marine Bands.

STAGE G.—*Upper Coal-measures* of Lancashire (2000 ft.) and
other English coal-fields. Red Sandstones &c., of Bothwell and
Ayr, in Scotland. Absent in Ireland.

STAGE F.—*Middle Coal-measures* of Lancashire &c., with prin-
cipal coal-seams (3000 ft.). Flat coal series of Scotland. Present
in Ireland (Tyrone, Kilkenny.)

Essentially Marine.

STAGE E.—"*Gannister Beds*" (Phillips), with marine shells and
thin coals (2000 ft.), in Lancashire. "Pennystone series" of Coal-
brook Dale, South Wales, &c. "Slaty black-band" series of Scot-
land. Present in Ireland (Kilkenny, Dungannon, Lough-Allen
coal-fields); also in Belgium, Rhenish Provinces, and Silesia, with
numerous marine shells.

STAGE D.—*Millstone-Grit series* of England and Wales. 3500 ft.
in Lancashire ; "Moorstone Rock" of Scotland ; "Flagstone series"
of Carlow and Kilkenny ; Millstone Grit of Fermanagh and Leitrim,
with coals and marine shells.

STAGE C.—*Yoredale Beds.* 3000 feet in Lancashire ; Upper
Limestones and "Lower Coal and Ironstone series" of Scotland ;
Shale series of Kilkenny and Carlow ; Ironstone shales of Lough
Allen, with marine shells.

STAGE B.—*Carboniferous Limestone.* Mountain Limestone of
Derbyshire ; "Scaur Limestone" in Yorkshire ; "Lower Lime-
stone" (Roman camp) of Scotland ; Carboniferous Limestone of
Ireland.

STAGE A.—*Lower Limestone Shale* of England. Calciferous Sand-
stone series ("Tuedian," Tate) of N. of England and Scotland ; Lower
Carboniferous Sandstone, N. of Ireland ; Lower Carboniferous slate,
with Coomhola grits, with marine shells, S. of Ireland. (In Scot-
land, estuarine or lacustrine.)

Palæontological Results.—On making a census of the Molluscan and other fossils from the various stages above that of the Carboniferous Limestone (Stage B) as determined by the Palæontologist of the Geological Survey, some interesting results were obtained, showing the prevalence of marine conditions up into stage E, and a general change in the character of the fauna in the succeeding stages. Including only the area of the British Islands, it was found that no fewer than 37 genera, with 74 species, of decidedly marine forms, occur in the Gannister-beds (Stage E), of which all the genera and about 40 species were known in the stage of the Carboniferous Limestone. The series includes *Phillipsia*, which has been found by Dr. F. Römer in the representatives of Stage E in Silesia.

On the other hand, of the whole number of species in stage E (Gannister beds) only 6 are known in the overlying stages F and G, these being characterized by the prevalence of bivalves of supposed lacustrine or estuarine habitats, variously called " *Unio* " and " *Anthracosia.*" Of the few species of marine genera known in stage F (Middle Coal-measures), about 5 or 6 species are peculiar to itself, according to the determination of the late Mr. Salter.

Such a remarkable difference in the fauna of the Upper and Middle Coal-measures, as compared with that of the Gannister beds, constituted, in the author's opinion, sufficient grounds for drawing a divisional line between these two divisions of the Carboniferous series. Of the several existing methods of classification adopted by different authors, none of them appeared sufficiently to recognize the palæontological distinctions and characteristics of the several formations. The large number of genera and species which are now known to range up from the Carboniferous Limestone into the Gannister beds, and no higher, indicated the proper horizon for a divisional line, in fact a palæontological break at the top of the Gannister beds.

On the other hand, the mineral and palæontological differences between the Carboniferous Limestone and the overlying Yoredale series * were sufficient to justify their separation into distinct divisions; while the Yoredale, Millstone-Grit, and Gannister series are related by close mineral and palæontological resemblances.

With a view, therefore, of bringing the classification of the Carboniferous series into harmony with the character of the representative faunas, and the physical features of the successive stages, the author suggests that stages C, D, and E, composed of essentially marine beds, should be united into a Middle Carboniferous group; while stages F and G would remain as at present, in the Upper Carboniferous, their fauna being essentially of freshwater. The series, as thus amended, would be as follows:—

Upper Carboniferous Group.

Stage G. Upper Coal-measures .... ⎫ Essentially
  ,, F.   Middle Coal-Measures .... ⎭ freshwater.

* In the south of Ireland there is strong evidence that the Yoredale beds ("Shale-series") are unconformable to the Carboniferous Limestone.

Middle Carboniferous Group.

Stage E.	Lower Coal-measures or Gannister Beds	}	Essentially marine.
„ D.	Millstone-Grit series		
„ C.	Yoredale series		

Lower Carboniferous Group.

Stage B.	Carboniferous Limestone series	}	Essentially marine (except in Scotland).
„ A.	Lower shales, slates, Carboniferous and		
	Calciferous Sandstone series		

The author then proceeded to show, by reference to the writings of Dr. F. Römer of Breslau, of M. De Koninck, M. Charles Barrois, &c., that stage E with its marine fauna, is represented both in Germany, Belgium, and France, as well as in the British islands, so that the classification would hold good over Western Europe, which was a sufficiently extensive area to justify the establishment of a distinct group of strata.

2. " On Coal-pebbles and their Derivation." By H. K. Jordan, Esq., F.G.S.

In this paper the author endeavoured to explain the mode of production of pebbles of coal in the clays and sandstones of the South-Wales Coal-field and elsewhere, the occurrence of which had been long since noticed by Sir William Logan and Sir Henry De la Beche. His opinion is that the pebbles in question are derived either from the seam of coal above which they are found, or from a seam of coal which formerly existed in the same, or approximately in the same position, and which has been destroyed by erosion, the effect of strong currents of water, which distributed the grains of sand and other materials upon the coal-seam.

LXXIII. *Intelligence and Miscellaneous Articles.*

ON THE EMPLOYMENT OF A SILVERED GLASS AS A CAMERA LUCIDA. BY A. TERQUEM.

EVERY one knows how fatiguing is the prolonged use of the cameræ lucidæ usually employed for drawing objects in relief or microscopic objects. Nevertheless this apparatus is very convenient when we wish to reproduce the outline of objects of which the perspective is difficult to obtain directly, such as certain physical apparatus: photography cannot always be employed, when the sketch is not to reproduce integrally the object itself with all its details.

I have found that, for the usual camera lucida with either one or two reflections, a glass semi-silvered by Foucault and Martin's process can be substituted with great advantage. For this purpose it is sufficient to leave the glass in the silver bath from one to two minutes at the most, according to the strength of the reducing agent and, especially, according to the external temperature, the influence of which on the reduction of silver is considerable.

I have made use of a simple plate of glass having a breadth of 1 decimetre and a length of 1·5. Semi-silvered glass has great reflecting-power, and yet remains very transparent; it presents merely a slight brown shade.

It is known that M. Foucault advised the investing with this semi-silvering the objectives of telescopes for viewing the sun, in order to arrest nearly the whole of the rays of obscure heat*.

When the glass is silvered, washed, and dried, the silver (which might be removed by the slightest friction) is fixed by coating the glass with a transparent varnish. For this purpose it is heated to about 40°, and the following varnish is poured upon the silvered face:—alcohol, 100 cubic centims.; mastic tears, 10 grams. The thin film of resin which adheres is very transparent and has a very even surface. The reflecting-power of the glass is slightly diminished, but is still sufficient. The silvered surface could be covered with another glass plate; but this would give rise to multiple reflection, which is avoided by using the varnish.

The glass is then fixed, when the varnish is dry, by one of its edges, in a nipper fitted to a foot, permitting various inclinations to the horizontal to be given to the glass; if the object to be drawn is vertical, the angle of 45° should evidently be preferred. The paper on which the drawing is to be made is fixed beneath. It is indispensable that above the glass a sight-piece be placed, to give the eye a perfectly fixed position. If the object has a strong relief, the images of the various parts are formed at different distances behind the glass, and the perspective changes with the position of the eye; it is the same with the coincidence of the points on the paper and the different parts of the image to be drawn. The sight-piece consists simply of a small slip of blackened cardboard pierced with a small aperture; this can be supported by the apparatus which sustains the glass.

If the illumination of the object, placed at a suitable height and distance before the inclined glass, be in a certain correspondence with that of the drawing-paper, the image of the object, the pencil, and even the line of the drawing as it is being executed can all be seen at the same time without any fatigue. The conditions of the illumination can be easily realized by the aid of screens or shutters.

The advantage of this camera lucida over that generally used arises from the reflection taking place over a large surface, which gives more intensity, and especially from the circumstance that the simultaneous visibility of the pencil and the image is independent of the position of the eye of the observer, depending only on certain conditions of illumination which can be easily regulated before commencing the execution of the drawing. It would be easy, by taking two parallel glass plates, one semi-silvered and the other having received a thick coat of silver, to make a camera lucida that

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* The same arrangement would be very advantageously employed in photographic enlarging-apparatus, where the solar heat sometimes cracks the plates.

could be fitted to microscopes, and more convenient than those at present employed.

NOTE ON THE SENSATION OF COLOUR. BY C. S. PEIRCE.

It may, perhaps, be worth while to notice a few consequences of three theories concerning colour which are usually regarded with some favour.

First Hypothesis.—The appearance of every mixture of lights depends solely on the appearances of the constituents, without distinction of their physical constitution. This I believe is established.

Second Hypothesis.—Every sensation of light is compounded of not more than three independent sensations, which do not influence one another. This is Young's theory. It follows that, if we denote the units of the three elementary sensations by i, j, and k, every sensation of light may be represented by an expression of the form

$$X i + Y j + Z k.$$

Third hypothesis.—The intensity of a sensation is proportional to the logarithm of the strength of the excitation, the barely perceptible excitation being taken as of unit strength. Negative logarithms are to be taken as *zero*. This is Fechner's law. It is known to be approximately (and only approximately) true for the sensation of light. From this it follows that, if x, y, z be the relative proportions of a mixture of three lights giving the elementary sensations i, j, k, the sensation produced by the mixture is

$$I \log x \cdot i + J \log y \cdot j + K \log z \cdot k,$$

where I, J, K, are three constants.

From these principles it follows that, if a light giving any sensation such as that just written have its intensity increased in any ratio r, the resulting sensation will be

$$I \log r x \cdot i + J \log r y \cdot j + K \log r z \cdot k$$
$$= I \log x \cdot i + J \log y \cdot j + K \log z \cdot k + \log r (I i + J j + K k).$$

Thus the result of increasing the brilliancy of any light must be to add to the sensation a variable amount of a constant sensation, $I i + J j + K k$; and all very bright light will tend toward the same colour, which may therefore be called the *colour of brightness*. Moreover, if the three primary colours be mixed in the proportions in which each by itself is just perceptible, the sensation produced will be

$$\log r (I i + J j + K k),$$

and can only differ by more or less.

Now I find in fact that all colours are yellower when brighter. If two continuous rectangular spaces be illuminated with the same homogeneous light, uniformly over each, but unequally in the two, they will appear of different colours.

If both are *red*, the brighter will appear *scarlet* ;

,,	,,	*green*	,,	,,	*yellowish* ;
,,	,,	*blue*	,,	,,	*greenish* ;
,,	,,	*violet*	,,	,,	*blue.*

If we have the means of varying the wave-length of the light which illuminates the fainter rectangle, we can improve the match between the two, by bringing the fainter toward the yellow. Such motions will converge toward a certain point of the spectrum which they will never cross—a point a little more refrangible than D and having a wave-length of 582.10^{-6} mm., according to Ångström's map. If both rectangles be illuminated with this light, the fainter appears white or even violet ; but if it be varied in wave-length with a view of improving the match, it will be found to return to the same point with the utmost precision.

It appears, therefore, that, if our hypotheses are correct, the colour $\log r$ ($\mathrm{I}\, i + \mathrm{J}\, j + \mathrm{K}\, k$) is like that of the spectrum at $\lambda = 582$, only that it contains less blue or violet, and is consequently of greater chromatic intensity.

It further follows from Fechner's law that, if any light be gradually reduced in brightness, one element of the sensation will disappear after another—and that when very faint it will exhibit only one primary colour, which is the one which it contains in greatest proportion relatively to the proportion in the light which has the colour of brightness. Now although this does not seem to be exactly the case, yet we do get some approximation to it. It is true that any light whatever, when sufficiently faint, appears white, owing to the self-luminosity of the retina. We cannot therefore, unfortunately, get sight of the primary colours by reducing the light of three parts of the spectrum. But we may, as has often been suggested, make use of the principle of contrast. If any red spectral light be sufficiently reduced, it will perfectly match any less-refrangible light. We may therefore say that a faint spectral red in contrast with a bright light of the same kind, excites with approximate purity one of the elementary sensations. The same thing is true of the violet ; and therefore a rich violet may be taken as another primary colour. In my book entitled 'Photometric Researches,' the printing of which is nearly complete, I show reason to think that the pure green has a wave-length intermediate between E and *b*. A faint green of this sort contrasted with a bright one appears as a very bluish green ; and this may therefore be supposed to be the third primary colour.

We have seen that it results from the theory that an increase in the brilliancy of any light adds to the sensation nothing of the peculiar colour of that light, but only a certain amount of the colour of brightness. If this be the fact, then the photometric sensibility of the eye should be the same for all colours. In order to ascertain whether this is so or not, I have made a series of determinations of my photometric probable error. Each determination was based on twenty-eight comparisons of two parts of the same-coloured disk. Since there were two unknown quantities

(namely, the relative brightness of the two surfaces compared, and an instrumental constant), it follows that only twenty-six observations were effective for determining the probable error. Let R be my photometric probable error of a single comparison. Then the probable error of a single determination of R (which we may denote by r) would be $\dfrac{\cdot 51}{\sqrt{26}} \times R$, or say $\frac{1}{10} R$. Having made a considerable number of such determinations of R with different coloured disks, let us ascertain their probable error from their discrepancies, considering them as so many independent observations of the same unknown quantity, and denote this probable error by r'. If, then, R really is the same for all colours, we should have

$$r' = r;$$

or at least the difference should not exceed ρ, the probable error of r', which may be calculated by the formula

$$\rho = \frac{\cdot 51}{\sqrt{mr'}},$$

where m is the number of sets of experiments diminished by 1. If, on the other hand, R varies with the different colours, and not merely accidentally, r' should have a larger value. The following are the values I obtained for R, the sum of the brightness of the two surfaces compared being taken as unity:—

		R.	Diff. from mean.
Feb. 6.	White	·0041	+·0001
	Red, just before C............	·0046	+·0006
	Chrome-yellow, A 2	·0032	−·0008
Feb. 7.	Red, just before C............	·0040	±·0000
	Staat's emerald green	·0046	+·0006
	Carmine, B	·0044	+·0004
	Chrome-yellow, A 1	·0037	−·0003
	Purple, Hoffmann's violet RRR	·0033	−·0007
Feb. 13.	Red, just before C............	·0048	+·0008
	Green, complementary to carmine	·0034	−·0006
	Blue-violet, No. 2	·0048	+·0008
	Yellow, A 1, mixed with black..	·0032	−·0008
	Mean	·0040	

After these experiments the method of observing was changed, and I obtained the following:—

		R.	Diff. from mean.
Feb. 14.	White window-shade, ill. by sun	·0030	−·0002
	Brown	·0030	−·0002
	Greenish sky-blue............	·0037	+·0005
	Very reddish purple	·0028	−·0004
	Yellow-orange	·0032	±·0000
Feb. 15.	" Fundamental green of Müller"	·0030	−·0002
	Vermilion, half between C and D	·0034	+·0002
	Violet....................	·2032	±·0000
	Yellow	·0036	+·0002
	Mean........	·0032	

We thus get from the

first twelve determinations, $r = \cdot00040$, $r' = \cdot00048$, $\dfrac{r'}{r} = 1\cdot2$,

last nine determinations, $r = \cdot00032$, $r' = \cdot00019$, $\dfrac{r'}{r} = 0\cdot6$,

and from the weighted mean, $\dfrac{r'}{r} = \cdot96$; so that it appears from these experiments that the photometric susceptibility of the eye is the same for all colours. The result, however, is uncertain, because it may be that R is chiefly due to other sources of error than the limitation of sensibility; still the experiments show as small a value of R as is usually obtained. I shall endeavour, by further observations, to obtain a conclusive result.

A further consequence of our hypotheses will be reached by differentiating the expression for a light-sensation. We have

$$d(\text{I}\log x \cdot i + \text{J}\log y \cdot j + \text{K}\log z \cdot k) = \frac{1}{x}dx \cdot \text{I}i + \frac{1}{y}dy \cdot \text{J}j + \frac{1}{z}dz \cdot \text{K}k.$$

Now, as x, y, and z all exceed unity, the differential is greater the nearer unity x, y, and z are. Hence, since the variation of the proportions of the primary colours with a variation of position in the normal spectrum is uniform, it follows that the change of colour of the normal spectrum should be most rapid about $\lambda = 582$, as it of course is. It is also obvious that, if the total quantities of the three colours are nearly the same in different parts of the spectrum (I here refer to these colours not as really objective, but as measured in the usual objective way), then the part about $\lambda = 582$ must be the brightest—another familiar fact.

I may observe that there is a modification of our formula for a sensation of light, which probably better represents the relations of the sensations. Writing, in the first place,

$$\mathbf{i} = \text{I}i, \quad \mathbf{j} = \text{J}j, \quad \mathbf{k} = \text{K}k,$$

the formula is

$$\log x \cdot \mathbf{i} + \log y \cdot \mathbf{j} + \log z \cdot \mathbf{k}.$$

This loses its validity when any of the logarithms become negative. If z is the smallest of the three quantities, we may substitute

$$\text{X} = \frac{x}{z}, \quad \text{Y} = \frac{y}{z};$$

and the formula becomes

$$\log \text{X} \cdot \mathbf{i} + \log \text{Y} \cdot \mathbf{j} + \log z \, (\mathbf{i} + \mathbf{j} + \mathbf{k}).$$

When x or y is smallest there will be two other formulæ. Now, as the variation in the brilliancy of the light affects only the last term of the last formula, and not the first two depending on X and Y, it is more than probable that the eye is habituated to separating the element of sensation which this last term represents, and which

is continually changing its values, from the rest which remains constant. It is therefore likely that the classification of light into three kinds, according as the *violet*, the *red*, or the *green* is contained in the smallest proportion, is one which has a relation to the natural powers of discrimination.—Silliman's *American Journal*, April 1877.

ON ACCIDENTAL DOUBLE REFRACTION. BY J. MACÉ.

The phenomena of accidental double refraction produced by compression have, since their discovery by Brewster, given rise to numerous researches, while those produced by tempering have been but little studied. Since the discovery of the latter by Seebeck, we find but little concerning them except the papers of M. de Luynes, in which the author studies chiefly the mechanical properties acquired through an energetic tempering, and those of M. Mascart on the tempering of cast plates of glass (*Journal de Physique*, 1876), in which the author applies himself especially to the practical side of the question.

The aim of the investigation which I commenced some months since was, on the contrary, to discover what are the laws which govern this phenomenon, and to search out the analogies or the differences which might exist between that which results from tempering and the other cases of accidental double refraction. I therefore proposed to myself to study plates of simple geometrical forms, especially rectangular and square, differing in dimensions, thickness, and constitution. If it be remarked that Wertheim, by the study of the regular compression of glass, has been able to show that the differences in the course are proportional to the pressures exerted, it will be seen that this led me to investigate the distribution of the differences of the course in the various plates submitted to experiment.

The arrangement employed was very simple :—A car carrying the plate permitted it to be displaced parallel to a micrometer formed, as in M. Jamin's apparatus, of two parallel fine threads and, besides, of a third, horizontal thread, rendered necessary by the curved form often (and particularly in square plates) affected by the fringes to be studied. As always, the polarizer was placed at 45°; the analyzer was alternately transverse and parallel, so as to exhibit fringes corresponding to differences of course varying by half wavelengths. The compensator was suppressed, it having the inconvenience of deforming the fringes and often rendering them difficult to observe; but it was made use of for the rectangular plates, in order to measure the central difference of course. For the illumination the burner of Laurent's saccharimeter was used. The position of each fringe was measured to within 0·1 of a millimetre at the least.

In studying the distribution of the course-differences along a line parallel to and equally distant from two of the sides of the plate, the phenomenon could in all cases be represented by a formula

identical with that representing the distribution of magnetism in a magnetized bar :—

$$y = A\left(K^x + K^{-x}\right),$$

reckoning x from the middle of the plate ; or

$$y = A_1\left(K^x + K^{2l-x}\right),$$

counting from the margin. The differences between calculation and observation were always less than the possible errors of observation, and only exceptionally rose to $0 \cdot 01$ of the measured length.

I will add that, in a long rectangular plate, for the distribution of the fringes parallel to the longer sides a curve was obtained similar to that of brachypolar magnets ; any square plate gives a similar curve to that of megapolar magnets. For example, there are found :—for a rectangular plate, $A = 2 \cdot 53$, $K = 2 \cdot 99$; for a square plate, $A = 0 \cdot 0124$, $K = 100 \cdot 5$. The ordinate-unit is the half of a wave-length ; the unit of length, the centimetre.

After these researches the question naturally arose, Are these phenomena to be attributed, as Neumann thought, to a regular action exerted upon the glass mass by the outer layers at the time of its solidification ? In regard to this I have made several series of experiments, varying the conditions ; but in all cases I was conducted to the same conclusion : the causes of these two phenomena are completely different.

It is quite otherwise when the effects of tempering are compared with those resulting from regular heating of a plate of glass through its contour. Examining with the polarizing microscope a square plate of glass heated by the usual processes, one is struck with the similarity of the phenomena. Unfortunately, the rapidity with which the colorations change and disappear did not permit precise measurements to be made ; I was therefore obliged to have recourse to an artifice.

I took two rectangular plates derived from the same plate of glass, and both having absolutely identical dimensions. One was feebly tempered ; the other could be introduced into a hot metallic piece constructed specially for the purpose. It was at once perceived that the signs of a tempered and of a heated plate are the same. If, moreover, after arranging upon the polarizing microscope the tempered plate at 45° from the plane of polarization, the heated plate of the same shape be superposed crossing it, a moment comes when the common part is traversed by a black cross, the arms of which pass through the four vertices of the square of intersection. This phenomenon can only be produced if the distribution of the retardations is at that instant the same in both plates. Double refraction produced by tempering is therefore identical with that which is produced by a regular heating through the contour of the plate.—*Comptes Rendus de l'Académie des Sciences*, May 7, 1877, tome lxxxiv. pp. 1024–1026.

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Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 6a.

Fig. 1.

Fig. 2.

Fig. 3.
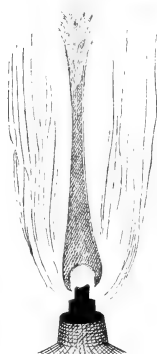
Fig. 4.

Fig. 5.

Fig. 6.
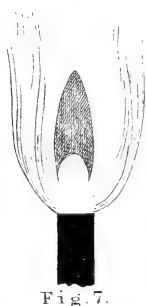
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Fig. 9.

Fig. 10.

Fig. 11.

Fig. 12.

Fig. 1.
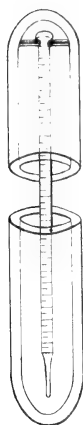
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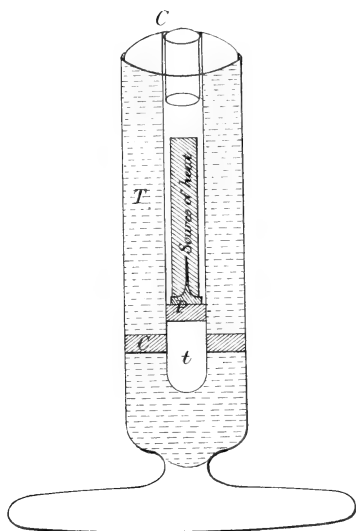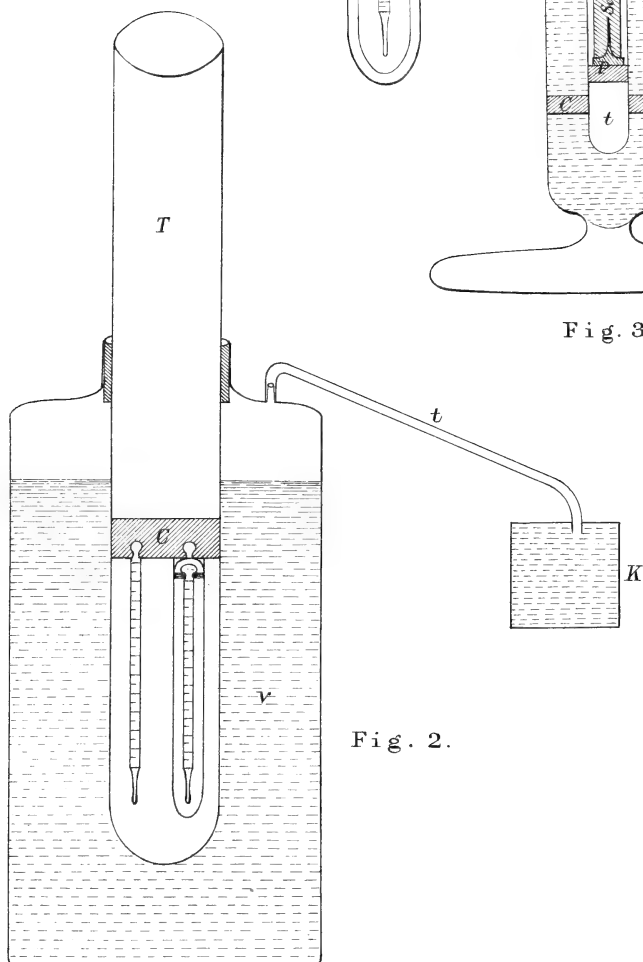
Fig. 3.

Barium Nitrate.

Fig.1.
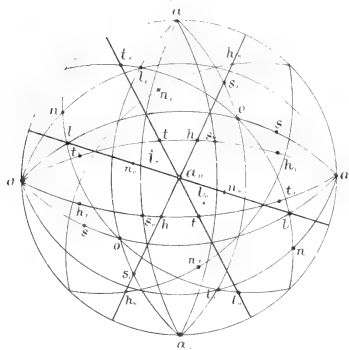
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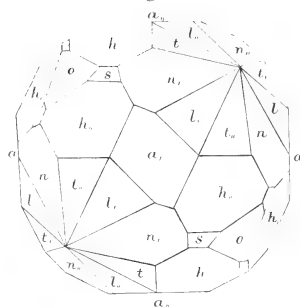
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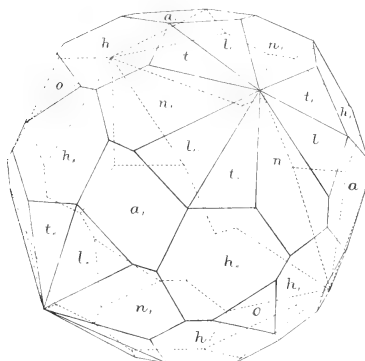
Sphene.

Fig.34. *From Hessenberg's Min.Not.VI.*

Fig.4.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

THE

LONDON, EDINBURGH, AND DUBLIN

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WITH TWO PLATES,

Illustrative of Mr. W. J. Lewis's Paper on Crystallographical Forms of
Glaucodote, and Dr. K. Heumann's on Luminous Flames.